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General Astronomy.

ON THE ATMOSPHERIC REFRACTION AT MADISON, WIS.*

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Investigations of the atmospheric refraction may be divided into two classes, one of which may be regarded as a department of theoretical physics, consisting of the mathematical investigation of the path of a ray of light traversing a gaseous medium of varying density, and resulting in an expression for the curve of the ray in terms of certain arbitrary constants. The other class of investigations is concerned with the experimental determination of the values of these constants, or, more precisely, in determining for the constants a set of values which will render as consistent as possible among themselves the observations made with a given instrument.

There seems to be no physical reason why such quantities as the index of refraction and co-efficient of expansion of air should not be the same in all parts of the Earth, when due allowance is made for such variable elements as temperature, gravity, etc., but the investigations of the second class, made at Greenwich, Paris, Königsberg, Pulkowa, etc., furnish quite different values of these quantities, and indicate as a probable conclusion that a certain amount of undetermined instrumental error has gone into them. While there can be no doubt that in the particular cases for which these investigations were made the erroneous values thus deduced have furnished a better system of reductions for the observations than could have been derived from correct values of the constants, this masking of instrumental errors renders the resulting refraction tables ill adapted to use under other circumstances or at other places; and imposes upon every observatory at which absolute declinations are observed the duty of redetermining for its own surroundings another set of values for these constants which shall so far as possible eliminate its own peculiar errors.

* Read at the Congress of ASTRONOMY AND ASTRO-PHYSICS, Chicago, August, 1893.

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I have made such an investigation for the meridian circle of the Washburn Observatory, from the best data accessible, following the classic method of determining the latitude from observations of stars at different distances from the pole, observed at both upper and lower culminations. These observations were reduced with the so-called Pulkowa Refraction Tables, which are constructed from Gyldèn's theory with two arbitrary constants deduced from observations with the Pulkowa vertical circle. Although theory indicates several minute corrections which should be applied to these tables in order to transform their data to the latitude of Madison, *e. g.*, those due to gravity, curvature of the meridian, humidity, etc., these corrections were neglected and the observations reduced with the tables unchanged. I have grouped the latitude results thus derived in accordance with the north polar distance of the stars employed, determining the limits of the zones, so that the partial results shall have equal weight. The several results, for the seconds of this latitude together with the results which would have been derived had Bessel's refractions been employed in the reductions, are shown in the following table:—

N.P.D.	Z.D.	Gyldèn.	Bessell.
1° - 8°	51°	36″.72	36″.54
10 - 13	58	36 .69	36 .50
14 - 19	63	36 .69	36 .48
19 - 24	68	36 .74	36 .51
24 - 31	74	36 .72	36 .45

The column headed Z.D. shows the average zenith distance of the stars observed below the pole. The observations were not arranged with reference to an investigation of the refraction and do not extend beyond 78° zenith distance.

The two refraction tables employed furnish appreciably different values of the latitude, but each of them produces an agreement among the individual results quite as great as could be expected from the probable errors of the data, and either table may therefore be employed for the derivation of a latitude with this instrument, its low latitude causing the different values of the constants employed in the tables to produce sensibly the same effect upon results from stars at different distances from the pole; but the two refraction tables will furnish quite different results for the declinations of southern stars *e. g.*, Bessel's refraction would place a star in — 30° declination 0″.7 further south than Gylden's, and the method ordinarily employed for an investigation of the refraction is unable to decide which system should be employed.

I have therefore resorted to another method for an investigation of the refraction,—the measurement of the angular distance between stars widely separated in the heavens. The instrument employed for this purpose is a modified form of the Loewy prism apparatus, attached to the six-inch Clark equatorial telescope of the Washburn Observatory. The principal element of the apparatus is a system of mirrors so placed before the objective as to reflect into the telescope images of the stars which are to be observed, both images being simultaneously visible, as in the case of sextant observations. In fact the apparatus may be regarded as a reflecting instrument employed like a sextant, but whose attainable precision bears about the same relation to the precision of sextant observations that its six-inch objective bears to the objective of a sextant telescope.

It is not my purpose to discuss here the theory of the instrument or its errors, and the methods employed for eliminating these defects, further than to say that these have been elaborately investigated, and that after all known sources of error had been taken into account, the measured distances between pairs of stars were compared with distances computed from the stars' cöordinates, right ascension and declination, and a system of very minute corrections arising from personal errors of the observer was determined whose application to the measured distances brought them into agreement with the mean of all the computed distances. All of the 76 stars observed are situated near the equator, and errors in their adopted declinations, therefore, have an exceedingly minute effect upon the computed distances, and as for errors in the adopted right ascensions, it is obvious that an error of equinox has no effect whatever upon the distance between a pair of stars, and that the effect upon the final results of all systematic error in the right ascensions is eliminated by the distribution of the stars throughout the twenty-four hours of right ascension. The mean of the computed distances can therefore be affected only by an accumulation of accidental errors, and as all of the stars have well determined places, the effect of these errors must be exceedingly small. Nevertheless even this residual effect has been in great part eliminated, by the observation of groups of stars making the complete circuit of the heavens.

The theory upon which the investigation of the refraction proceeds is exceedingly simple. The atmosphere constitutes a hollow sphere which bends every ray of light entering it obliquely, and diminishes the angular distance between every pair of stars above the horizon. The analytical expression for this

effect of the refraction in terms of the ordinary refraction theory is comparatively simple, and equating this expression to the difference between the measured and computed distances separating the stars, we obtain a series of equations from which the constants of the refraction theory may be derived.

The available data of this kind are 812 observations of the distances between 38 pairs of stars. All of these observations were made when the stars comprising a pair were at approximately equal distances from the zenith, and these zenith distances ranged from 64° to 73°. All of the investigation and all of its results, therefore, relate to zenith distances included within these limits, and the conclusions here presented are not definitive, since the right ascensions of a few of the stars are still under investigation.

There are some subsidiary matters which it will be necessary to consider before proceeding to the absolute values of the constants with which this investigation is chiefly concerned, and chief among these is the possible effect of humidity of the atmosphere upon the refraction.

The only investigation known to me of the effect upon the refraction of the presence of aqueous vapor in the atmosphere is that of Laplace contained in the Mécanique Céleste. Laplace derives numerical values for the effect of humidity, and after tabulating them remarks: "*Il resulte de cette table que l'effet de l'humidité de l'air sur la refraction est tres-peu sensible, l'exces de la puissance refractive de la vapeur aqueuse sur celle de l'air étant compensé en grande partie par sa plus pétite densité.*" *Méc. Cél., Vol. IV, Book V, Chapter 1.* This remark of Laplace's seems to be at least partially justified by an investigation of C. A. F. Peters, who found that observations of Polaris with the Pulkowa vertical circle, when made through clouds, furnished a latitude 0″.04 greater than that derived from observations made in a clear sky. Recueil de Mémoires, etc., par W. Struve, Vol. I, p. 142.

I know no other foundation than the above for the common practice of assuming the effect of humidity upon the refraction to be insensible, and the evidence thus adduced seems open to the following criticism: The observations of Peters which were made through clouds were distributed throughout the year, and their effect upon the latitude was discussed without any reference to the temperature at which they were made. It is apparent that the effect of humidity will be most pronounced at high temperatures, and during the winter months will have but a small

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fraction of the maximum effect which it attains in the summer. Peters' discussion, therefore, seems little adapted to bring out the real effect of the humidity, and in justice to him it should be stated that he does not appear to have intended to investigate this effect, since he expressly states: "*Il faut voir si les nuages, surtout par leur influence sur les temperatures de l'air, ne tendent pas à changer les refractions,*" *etc.*

As for the investigation of Laplace, it is based upon the theoretical physics of his day, and involves assumptions which are not tenable at the present time. In particular, his value of the index of refraction of aqueous vapor is derived from the index of refraction of water by means of the assumed relation

$$n^2 - 1/d = c$$

where c is a constant, n denotes the index of refraction and d the density of water, whether in the liquid or the gaseous state. The equation is a direct consequence of the emission theory of light, and although a certain amount of countenance is given to it by the undulatory theory, it is not necessarily involved in the latter and the experiments of Gladstone, Dale, and others, discredit it. In particular it should be noted that the equation in the hands of Laplace furnishes for aqueous vapor a refractive power greater than that of air, while laboratory experiments furnish a smaller value for its index of refraction. Thus the *Annuaire du Bureau des Longitudes* for the year 1892 gives, upon the authority of Mascart, the value 1.000257, while in Mascart's *Traité d'Optique* the value is given as 1.0002574, and attributed to Fizeau. The index of refraction of dry air is given by Mascart as 1.0002945. From the standpoint of modern physics, therefore, the refraction should be less in a humid than in a dry atmosphere, while Laplace's corrections tend in the opposite direction.

To determine the effect of the aqueous vapor corresponding to the numerical values above given, I denote by n_1 and n_2 the indices of refraction of air and aqueous vapor respectively at a temperature of t_0, C., and pressure of 760 *mm*, and denote the respective co-efficients of expansion by m_1 and m_2. The refraction corresponding to any zenith distance z is then given by the equation

$$R = \frac{n_1 - 1}{\sin 1''} \cdot \frac{b_1}{760} \cdot \frac{1 + m_1 t_0}{1 + m_1 t} \tan z + \frac{n_2 - 1}{\sin 1''} \cdot \frac{b_2}{760} \cdot \frac{1 + m_2 t_0}{1 + m_2 t} \tan z$$

in which the two terms of the second member represent respectively the refractions produced by the air and by the aqueous vapor at pressures of b_1 and b_2 *mm* respectively.

The co-efficient

$$\frac{n_1 - 1}{\sin 1''}$$

is commonly called the constant of refraction, although it is slightly variable with the zenith distance, and

$$\frac{n_2 - 1}{\sin 1''}$$

should in strictness also be considered variable, but since it is multiplied by the small factor

$$\frac{b_2}{760}$$

its variations will be neglected, and the variation of the first co-efficient will be assumed known from the ordinary refraction theory. Let b represent the total atmospheric pressure

$$b = b_1 + b_2$$

and we have

$$R = \alpha \frac{b}{760} \cdot \frac{1 + m_1 t_0}{1 + m_1 t} \tan z - \frac{b_2}{760} \left\{ \frac{n_1 - 1}{\sin 1''} \cdot \frac{1 + m_1 t_0}{1 + m_1 t} - \frac{n_2 - 1}{\sin 1''} \cdot \frac{1 + m_2 t_0}{1 + m_2 t} \right\} \tan z.$$

The first term in the second member of the equation is the ordinary expression for the refraction; the second term constitutes a correction to the refraction which may be represented by ΔR. To express this term in a simpler form let us put

$$\frac{(n_1 - 1)(1 + m_1 t_0)}{\sin 1''} = \mu_1 - 1 \qquad \frac{(n_2 - 1)(1 + m_2 t_0)}{\sin 1''} = \mu_2 - 1$$

$$\mu_1 - \mu_2 = \omega \qquad (\mu_1 - 1) m_2 - (\mu_2 - 1) m_1 = \psi$$

and obtain

$$\Delta R = - \frac{b_2}{760} (\omega + \psi t) \tan z$$

To derive numerical values for ω and ψ I adopt the following values of the refractive indices and co-efficients of expansion of air and aqueous vapor

$n_1 = 1.0002945$	Biot et Arago.
$n_2 = 1.0002574$	Mascart, Fizeau.
$m_1 = 0.003670$	Regnault.
$m_2 = 0.004187$	Hirn.

and find after dividing by 760

$$\Delta R = - b_2 \{ [8.0032 - 10] + [5.0273 - 10] t \} \tan z$$

the square brackets denoting that the numbers placed within them are logarithms.

By means of this expression the effect of the humidity at any zenith distance may be computed, and a comparison of observations made at high temperatures, but under different humidities, will suffice to test the reality of the correction. I have made such a comparison as follows: From the observations with the prism apparatus I have selected all those which satisfy the following conditions: The same pair of stars must have been observed at a temperature above 15° C., when the relative humidity was (a) greater than 80, (b) less than 60. Designating the observations (a) by the symbol W (wet) and the observations (b) by D (dry) I have formed for each pair of stars the mean difference W — D. There are in all 70 observations of 15 pairs of stars available for this comparison, and from these I find

Uncorrected	$W - D = +0''.17 \pm 0''.08$
Corrected	$W - D = -0\ .03 \pm 0\ .06$

Although the quantities involved are small there seems to be little doubt that the effect of the humidity is sensible. and that it is correctly represented by the adopted formula. I have therefore applied this correction to all of the observations, and in what follows they are therefore to be considered as observations made in dry air.

The general character of the effect of humidity upon observations of zenith distance, reduced in the ordinary manner, may be shown as follows: The aqueous vapor tension of a saturated atmosphere within the range of temperature with which we are concerned may be represented by an exponential function of the temperature, and since the mean value of the relative humidity is nearly constant at all seasons we may write approximately

$$b_2 = \lambda 10^{\mu t} \qquad R = (\alpha\beta\gamma - f\lambda 10^{\mu t})\tan z$$

The numerical value of λ varies with the climate, being greater near the sea than in the heart of a continent. For Madison, where the average relative humidity is 75 per cent we have when t is expressed in degrees C.

$$\lambda = 0''.03 \qquad \mu = 0.030$$

It is evident that at low temperatures the effect of humidity will be quite small, but that it increases very rapidly with rising temperatures. The effect of this correction term is in part taken into account in the Pulkowa refraction tables by employing an erroneous co-efficient of expansion of air somewhat greater than the true value, but in Bessel's tables too small a value of this coefficient is adopted, and the humidity effect, instead of being re-

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duced, is considerably magnified in these tables. Bessel in fact appears to have been misled by Laplace into supposing that the effect of humidity was to increase the refraction, and that the coefficient of expansion which he determined from his observations ought therefor to be smaller than the true value for dry air, since he says in explanation of its difference from the value found by Regnault that it "*medio cuidam humorisstatui respondet.*" *Tab. Reg., p. LX.*

By means of the two constants involved in α and γ of the refraction formula, the computed refractions may be made to agree with observation at any two temperatures, but if the humidity term be neglected they cannot agree at more than two points of the temperature scale, but will be too small at intermediate, and too great at extreme temperatures. It follows therefore that zenith distances reduced with the ordinary refraction tables should show a periodic variation running through its cycle in a period of a year. In the case of the Pulkowa tables the amplitude of this variation scarcely exceeds 0″.01, but for the Bessel tables its amount is approximately 0.″07 tan z.

To exhibit the effect upon the refraction of the fluctuations in the humidity which take place from day to day I assume a zenith distance of 75° and a temperature of 21° C., and compute the humidity correction on two hypotheses, (a) that the α and γ of the refraction formula are derived for dry air, and represent the best attainable values of the physical constants involved; (b) that the α and γ are so derived from observation that they exactly represent the refraction at the given temperature for a mean condition as regards humidity. The results are as follows:

Relative Humidity.	50	60	70	80	90	100
ΔR (a)	−0″.36	−0″.43	−0″.50	−0″.57	−0″.64	−0″.71
ΔR (b)	+0 .18	+0 .11	+0 .04	−0 .03	−0 .10	−0 17

These series of numbers may be considered as representing extremes between which the actual correction for a similar case may be expected to lie, and they lead to the conclusion that the humidity cannot safely be neglected in the refraction computations if it is desired to take tenths of a second into account.

The expression above given for ΔR may be transformed by means of the development of $log(1-x)$ into a form much better adapted to computation with the ordinary tables. The result of this transformation is as follows: Let there be substituted for the γ of the tables the expression

$$\log \gamma = \log G + \log H$$

where G is the old γ recomputed with the co-efficient of expansion for dry air, and H is a function of the aqueous vapor pressure, b_2 *mm*, given in units of the fifth decimal place by the equation

$$\log H = -8.2\, b_2$$

The refraction computed with this value of γ will be substantially the same value that would have been obtained by the subtraction of ΔR.

Having corrected for humidity the observations made with the reflecting apparatus, it became necessary to derive a new value of the co-efficient of expansion of air, which was done by a comparison of distances of the same pair of stars observed at high and low temperatures. This determination is complicated by the necessity for taking into account the effect of changing temperature upon the instrument as well as upon the atmosphere, but fortunately the observations are sufficient to separate well these effects. The data available for the determination consist of 208 observations equally distributed between high and low temperatures, the average difference of temperature being about 15° C. I have not employed the method of least squares for the discussion of these data, but have grouped the equations resulting from the several observations in such a way as to eliminate the absolute distances between the stars and to produce maximum co-efficients for the correction to the co-efficient of expansion adopted in the Pulkowa tables. The resulting normal equations are

$$\begin{aligned} 43.33x - \ 6.88y &= +14''.42 \\ 3.13x + 33.92y &= +14\ .72 \end{aligned}$$

where x relates to the effect of temperature upon the atmosphere, and y to the corresponding effect upon the instrument.

The relation of x to the co-efficient of expansion of air is as follows: The temperature determinations which accompanied the observations were made by whirling a thermometer in the open air just outside the dome containing the instrument. Suspecting that although the errors of the thermometers were well determined, these observed temperatures might be affected by some error depending upon the mode of exposure, I carried on a simultaneous series of readings of thermometers placed in a closed chamber near the objective of the telescope, and ventilated by an exhaust fan which drew a gentle current of air from outside the chamber over the bulbs of the thermometers. The apparatus is substantially the same as Assman's Aspirations-Psychrometer, although I knew nothing of this instrument at

the time of its construction, and while its indications may not be absolutely free from error, they appear to me far more reliable than any temperature determinations made with an exposed thermometer, and I adopt its indications as a normal system of temperatures. From a least square solution of the comparisons made between the ventilated and whirled thermometers I obtain the relation

$$V - W = +\,0^{\circ}.015\,(t - 17^{\circ})\ \text{Centigrade},$$

i. e., the true temperature may be represented as a linear function of the readings, W, of the whirled thermometer. Putting $t = \alpha + \beta W$ and denoting by m^1 the co-efficient of expansion of air adopted for the γ factor of the Pulkowa tables, and by m the corresponding co-efficient to be determined from the observations, we have

$$x = 10000(m\beta - m^1)$$

and from this equation, with the value of β experimentally determined, I find

$$m = 0.003674 \pm 8$$

the probable error including the uncertainty in the determination of β as well as x. The close agreement of this value of m with Regnault's classical determination 0.003670 furnishes a partial proof of the substantial accuracy of the several elements which have gone into its determination, and confirms the correctness of the temperatures furnished by the ventilating apparatus.

The values of x and y derived from the normal equations have furnished a set of corrections to the observed distances between pairs of stars by application of which these distances have been reduced to what would have been obtained had the observations been properly corrected for the effect of temperature upon the instrument, and reduced with the above value of m, or with substantially Regnault's value of m, since the difference between the two in no case produces a difference as great as 0″.1 in the distances.

The observations thus corrected furnish a body of data which I have compared with the Pulkowa tables, corrected for the difference in gravity at Pulkowa and Madison by the subtraction of 0.00064 from log μ. The result of that comparison is that after the Pulkowa refractions have been corrected for gravity, for humidity, and for the changed value of m they require a further correction of + 0.00033 to log μ, or the refraction at 45° zenith distance should be increased 0″.04. If the humidity cor-

rection be neglected, and the temperature factor of the tables retained unchanged, the correction to log μ will be a variable quantity whose average value is approximately — 0.00025. The difference between Bessel's α and the μ of the Pulkowa tables is not constant, but its average value within the present limits of zenith distance is log α — log μ = + 0.00115, and the corrections to log α are therefore — 0.00082, when the humidity and temperature corrections are applied as above, and —0.00140 when they are neglected.

The degree of confidence to be accorded these results depends upon the judgment that is formed with regard to the precision of the observations. The results derived for the effect of humidity and for the co-efficient of expansion of air being in close accord with the results of laboratory experiments, conducted under a much wider variation of conditions than can be attained in astronomical observations, tend strongly to show the freedom of the observations from any considerable systematic error, save possibly a constant error affecting all of the observations of a pair alike. The agreement among the results furnished by the separate pairs of stars is shown by the small probable error of the correction to the co-efficient of expansion of air, and farther by the probable error of a single observation, ± 0″.31, which is less than the probable error assigned by Bessel and Gyldèn to the computed refractions corresponding to the zenith distances at which the observations were made.

The above presentation is to be considered as a partial abstract of results. The observations together with a description and theory of the apparatus employed will be set forth in detail in the Publications of the Washburn Observatory.

PHOTOGRAPHIC OBSERVATION OF MINOR PLANETS.*

MAX WOLF.

The difficulty in finding a faint minor planet in order to get an observation of it, is a great one, because the position of the planet is usually but roughly known. Even with a good ephemeris it is often a very fatiguing work in consequence of the want of charts for the fainter stars. The observer must make a diagram of the region in which he supposes the planet to be. After

* Read at the Congress of Astronomy and Astro Physics, Chicago, August, 1893.

a time, on comparing the diagram star by star with the sky, he finds that one star—the planet sought for—has moved. But if the ephemeris was very far from the true one, the whole work would be lost, because one would have taken up a false part of the sky.

A much greater work is to be done, if one wishes to detect a *new* minor planet. There are a hundred and more fields of view to be charted and compared, before a new one is found. The perseverance of the planet-hunter can not enough be admired.

Photography holds out—as will immediately be seen—two great advantages over this ancient method; first, it gives us a larger field and thus increases the probability of catching a planet about fifty times; then it permits us to distinguish the planet from the enormous number of faint stars, the planet moving amongst the stars and marking a short trail on the photograph.

I commenced photographing asteriods in 1890 using two instruments, a telescope lens of 16.2 cm. aperture and 262 cm. focal length, and an aplanatic lens of 6 cm. aperture and 44 cm. focal length. But I had no success, because I did not employ suitable lenses. The focal length of the first employed being too long, and the aperture of the second, too small.

(*a*). To photograph *faint* minor planets, a marked brightness of image is required.

For photographing nebulæ the brightness of image has as a factor approximately the quantity $\frac{D^2}{F^2}$, because the brightness of the image is diminished if the area is enlarged.*

A minor planet gives as area a "point." Comparing different lenses the brightness of the image would be in proportion to the square of the diameter: D^2, or more accurately in proportion to $\frac{D}{\sqrt{F}}$. But because the train of thought is the same for the two formulæ, we take for simplicity's sake the first.

The planet is moving during the time of exposure. Pointing the plate upon the fixed stars, we get a trail on the plate from the planet; and this trail becomes longer, when using a lens of longer focus. The intensity of the planetary image therefore is diminished by a longer focus, and we have approximately for the intensity: D^2/F (or perhaps more correctly, $D/F^{\frac{3}{2}}$).

To photograph minor planets, then, we need a lens with an

* Where D is the diameter and F the focal length of the instrument.

aperture as great as possible, with a focal length as short as possible, and last but not least one giving a large field.

Mr. Roberts who has first photographed the minor planet Sappho, used his mirror of 51 cm. aperture and 254 cm. focal length. The intensity of his instrument was therefore 10.2. My first instruments gave an intensity only of 1.0 and 0.8, and therefore I had no success for fainter planets.

From this point of view, after having comprehended the matter I re-commenced the photography of minor planets in November, 1891, using an aplanatic lens, which had $T = 2.2$, which is still four and a half times feebler than Mr. Robert's. But it was strong enough and had a field twenty-five times larger than that of Mr. Roberts.

By this lens I got the first *new* minor planets discovered by means of photography. Subsequently I used a 6-inch portrait-lens. The intensity of which was 3.5, and the field about 100 square degrees.

A large portrait lens is the most efficient because D^2/F is there at a maximum and the field is also as large as possible. Reflectors therefore, are not useful in photographing minor planets, since their field is too small.

(*b*) To have success in photographing minor planets there are some other points on which care must be taken.

Very sensitive plates must be used. The development is to be made without haze; they must remain as clear as water. If not, the faint planetary trails are lost. For this reason moonshine is a very great impediment.

Photographic operations have to be finished as quickly as possible, in order that the examination of plates take place before the planet has moved too far.

There are a large number of objects on the plate which are similar to a planetary trail as close double stars, lines of nebulæ, and especially those faint stars, which are so remarkably pressed into laces. From *one plate alone*, therefore, the certain detection of a planet is seldom possible.

In order to ensure complete success, we take a second plate immediately after the first. Then every planetary trail is recognized by the rule that the trail on the second plate must be the continuation of the trail on the first plate, and that the spot where the planet was trailing on the first plate must be clear on the second, and *vice versa*.

Another way is the following: We use two similar pieces of apparatus, mounted together. With the one we expose a plate for

the first and second hour; with the other for the second and third hours. In this way we need only three hours, instead of more than four by the first method.

(*c*). To take accurate positions from the plate there are several ways: for example by rectangular coördinates or by the parallactic apparatus, etc. I used my own method, because I was compelled to use the cheapest apparatus. I measure in the field of view of a microscope by a filar-micrometer the distances of the middle of the planetary trail from three or more comparison stars. From those distances I obtain by the formulæ of trigonometry the differences of α and δ.

Since a number of comparison stars can be taken, the position is less influenced by erroneous positions of comparison stars, than in ordinary optical work; and likewise errors of measuring and reckoning are checked by taking several stars, which is easily to be done in day-time.

(*d*). The question may now arise why we use the photographic method for position work—and not for planet-seeking only.

Indeed so far as regards my small portrait lenses—also Mr. Charlois'—the operation of measuring a planet, *of which the place is given*, is for the most part much more conveniently done by optical work with a large telescope, because the long fatiguing exposure is not required. But increase the photographic lenses by a few inches, and they will photograph planets invisible even through a large telescope. It has already often happened, that one of the smallest planets obtained by photography was below the optical power of a 12-inch Clark refractor.

If we can use larger portrait lenses, then we *must always* measure the positions by means of photography.

(*e*). There are some problems to be examined, which are connected with the measurement of the planetary trails, one of which I shall indicate.

In measuring the position of a planet on a plate, the short trail must be bisected. Now it would appear that the time of the middle of the exposure is that of the middle of the trail. But this is not necessarily true. The chemical process and the variation of the clearness of the sky can displace the middle of the trail.

It is possible that from chemical reasons the trail is prolonged a little at the end of the exposure. The simplest way to find it, was simultaneous eye-observations and photographs. But I had not the optical means to do so. I commenced to compare the

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trails and points of a star cluster taken on the same plate. This very small prolongation, if it is there, must be easily found as a function of the velocity of movement and of the brightness of the star.

The second error is more troublesome. Even if we suppose, that the status of the atmosphere would remain the same during the exposure,—the influence of the absorption will displace the middle of the trail. For, if we expose upon a planet rising to its culmination, its impression upon the plate will first be very faint—perhaps too faint for any impression,—and towards the end, stronger and stronger, and then still further enlarged by the phenomenon of so-called photographic irradiation. Bisecting the resulting trail by our micrometer wire, we get a place, which belongs to a later time than the middle of the exposure.

Since it is only a question of time, when nearly all observations of faint minor planets will be made by photography, it is very desirable, that such problems are given our careful attention.

CHICAGO, August, 1893.

THE BUREAU OF MEASUREMENTS OF THE PARIS OBSERVATORY.*

DOROTHEA KHEMPKE.

Under the presidency of Rear-Admiral Mouchez, the International Astro-Photographic Congress was held in the years 1887, 1889, 1891 at the Paris Observatory.

At that time there was planned in minutest details the great undertaking of the "Chart of the Heavens." Upon this work, at the present moment, eighteen observatories are busily engaged, all working upon the same plan, the same scale, with similar instruments.

In a most useful and interesting memoir, Mr. Trépied, Director of the Algiers Observatory, has classified and commentated on the various resolutions adopted by the three successive congresses above mentioned.

For purposes of reference I present the "Index" of Mr. Trépied's memoir (translated from the original text).

1. Photographic Plates.
2. Distribution of the Centers of Plates.
3. Selection of Guiding-Stars.

* Presented at the Congress of ASTRONOMY AND ASTRO-PHYSICS, Chicago, August, 1893.

4. Use of the "*Réseau*" (horizontal lines crossing vertical lines traced on a silver plate).
5. Focusing of Photographic Instruments.
6. Orientation of Plates.
7. Limits of Star Magnitudes for the Two Series of Plates (Catalogue and Chart).
8. *Modus operandi* of the Plates of the Catalogue.
   Modus operandi of the Plates of the Chart.
9. Developing of Plates.
10. Preservation of Plates.
11. Inscriptions on Plates.
12. Measurements and Instruments of Measurement.
13. Reproduction of Plates.
14. Distribution of the Work between Coöperating Observatories.

PHASES OF THE BUREAU.

In September, 1891, the regular work of the Chart of the Heavens and of the Catalogue was begun at the Paris Observatory. One hundred and thirty-three plates were taken up to January 1892. The following year (1892), the work was somewhat delayed on account of the construction of the new laboratory and the new pavilion for the "Bureau of Measurements," notwithstanding MM. Henry have been able to take 82 photographs of the Chart (exposure 40 minutes) and 173 photographs for the Catalogue.

1889.—Formation of one or more bureaus.

The moment had come to utilize the stellar photographs.

The Congress of 1889 had appointed a committee of seven members to study the question in all its bearings, to devise a plan for the proper construction of instruments of measurement. Also at this time, the Congress recognized the desirability of creating one great bureau embracing within itself all necessary requirements, or several minor bureaus in case the observers should find themselves unable to measure their own photographs.

1891.—In 1891, the committee formulated the following resolution (No. 12, translated from the French):

"As soon as may be judged convenient, each observer will make or have made by whatsoever Observatory or Bureau of Measurements he may choose:"

1st. "The measures, in rectilinear coördinates of the positions of the stars of the Catalogue (each star being measured with reference to the nearest lines of the *Réseau*).

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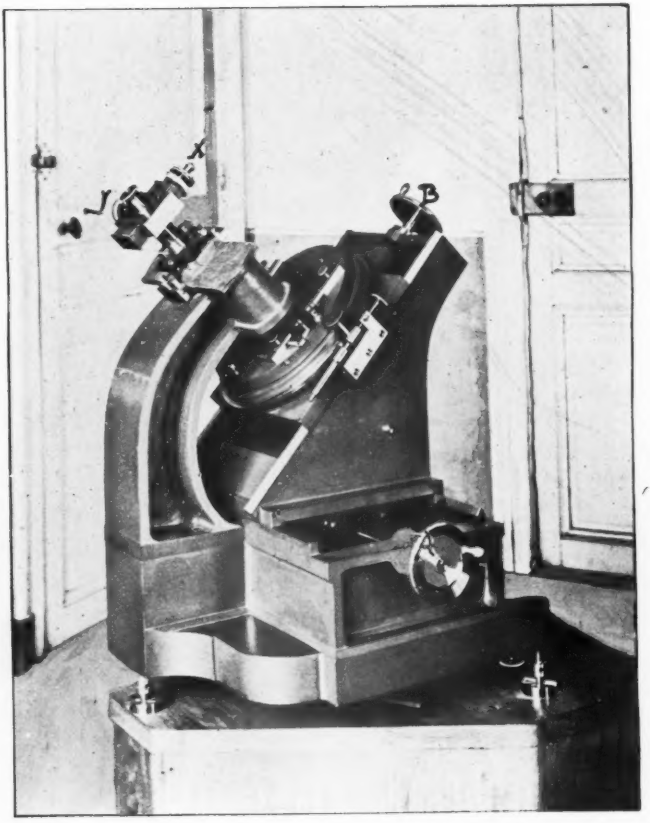
PLATE XL.



Apparatus for Measuring Photographic Plates at the Paris Observatory.

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2nd. "The necessary measurements for the determination of stellar magnitudes."

Resolution No. 12 established that, in general, the stars of the plates of the Catalogue should be measured in reference to the lines of the *Réseau*; however, it excludes no special method of measurement, (*viz.* see *Monthly Notices*, 1893, measuring apparatus at Greenwich Observatory.)

For the parts of the heavens extremely rich in stars, Dr. Gill and Professor Kapteyn have contrived a parallactic instrument by means of which measures can be made rapidly and with sufficient accuracy. (See *Bulletin de le Carte du Ciel.*) Through the generosity of M. Bischoffsheim, the Paris Observatory has been endowed with a parallactic instrument which has recently been constructed by M. P. Gautier.

A simple and more convenient apparatus, has been devised for the Bureau of Measurements at the Paris Observatory.

Bureau of Measurements.

The Bureau of Measurements was organized at the Paris Observatory early in February 1862. Its present staff: Dorothea Khempke, Directress; Assistants: Wm. Schott, Mlles. Thomy, Duguen, Marquett, Coniel.

Herewith are sent two views of said Bureau one view of instruments.*

Measuring Instruments—Description. The measuring instrument is similar in many respects to the macro-micrometer of MM. Henry, which was constructed by M. Gautier in 1884. With this instrument the Paris Observatory made its first measures of photographed binary stars. (See *Rapport Annuel*, 1885.)

The instrument of the Bureau of Measurements has been described by me in the *Rapport Annuel sur l'état de l'Observatoire de Paris pour l'année 1892.* Translation can be found in the *American Journal*, Astronomy and Astro-Physics, of June, 1893.

Description. The instrument is composed of three principal parts.

1st. A fixed horizontal piece.

2d. An inclined plane upon which is placed the plate under examination.

3d. A curved piece supporting the little telescope. Each plate when once in position is susceptible of three motions.

1st. A rotary motion, turning to set plate in invariable direction.

* A photograph of one of the instruments only accompanied this article. See August Astronomy and Astro-Physics, page 590.

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2d. A rectilinear motion of plates on the inclined plane.

3d. A rectilinear motion of the inclined plane on the horizontal plane.

* * * * * * * * * * * * *

Each instrument (at present the bureau possess two) is handled by two assistants taking turns; one making settings, the other recording measures. The stars of the plate under examination are measured by the two observers. Each makes one setting on the two images of the stars, and one setting on each of the four lines of the *Réseau* comprising it.

In the measnrements of the plates the lines of the *Réseau* play most important part.

The Congress of 1887 decided that each photographic plate should bear the impression of a system of lines derived from a *Réseau*, by means of which lines all errors produced by distortion of the film should be eliminated.

The lines of the *Réseau* are 130 mm. long, distant from each other by 5 mm. Thus each plate presents 67.6 squares, easily read on the plate as well as on the apparatus when in place.

By means of two screws, A, B, each 18 cm. in length with pitch of 5 mm., the 67.6 squares of the plates are successively brought into the field of the little telescope. Further, by means of two micrometric screws, X, Y, also of two double spider threads, the distance of the stars in any one square is measured in reference to the lines. The centre of each plate and the value of one revolution of the screw are ultimately determined by the positions of fundamental stars photographed on the plate.

In order to eliminate false stars which may appear on the plates after the *Réseau* is once impressed upon them, the conference of 1892 thought it wise to recommend two exposures on the same plate, one of 5^m the other of $2^m\ 30^s$, the distance between the two images of any one star being from $\frac{2}{10}$ to $\frac{3}{10}$ of a millimeter. Thus each plate of catalogue bears two impressions of each star individually measured by placing the double spider-threads at an equal distance from the center of the disc.

In the plates recently executed, one finds a third photographic image of stars which corresponds to an exposure of 20^s made upon the proposal of Mr. Christie. These short impressions are measured in case of bright stars. They are useful in determining stellar magnitudes, in testing the transparency of the atmosphere.

The measures of one star are composed of sixteen settings.

The photographic images are well defined generally, each disc

being of equal intensity within a circle of two degrees. In the corners of the plate they are elongated, presenting the shape of an ellipse.

SOME MEASUREMENTS MADE IN THE BUREAU OF MEASUREMENTS.

The stars, in number one thousand, found on the plate Nos. 187, 185, 201, 202, 203, were measured and reduced in the Bureau of Measurements during the year 1892.

Their final results were used and discussed by M. Loewy in order to test his new method of *raccordement* (overlapping).

M. Loewy comes to the following conclusion:

"The discussion of the plates 187, 185, 203, 202, 201, shows that the total error affecting the relative positions of a star does not exceed ± 0″.11. In this very small number are comprised all the atmospheric and optical influences and effects that may modify the apparent position of two neighboring stars, the distortion of the plates and of the film, the imperfections of the *Réseau*, all instrumental errors, accidental errors of measurements and personal equation in settings."

THE WORK OF THE BUREAU OF MEASUREMENTS.

During the first year of the Bureau there have been measured 2,600 stars, viz., 1,440 from the 23d of February to October 29; 1,160 from October 31 to December 30. Early in November the method of observation was considerably simplified, the number of settings on each star being reduced from eighty to sixteen.

The second apparatus was delivered on the 20th of March.

The stars measured during the current year with both instruments are 9,618 in number. 5,384 having been measured with the first apparatus and 4,334 with the second.

Thus from Feb. 23, 1892, to July 26, 1893, the Paris Bureau of Measurements of the Paris Observatory has determined the positions of 12,218 stars.

With this number compare those of the following catalogues:

Flamsteed	3310	Piazzi	7646
La Caille	9776	Rümker	11978
Bradley-Bessel	3222	Baily	8377*

Should the Bureau of Measurements continue its work under the present conditions, it would be able to measure in two years $\frac{4}{10}$ the number of stars contained in Lalande's Catalogue, in five years $\frac{12}{10}$ those of Weisse's Catalogue; in three years $\frac{7}{10}$ those of Cooper's Catalogue, in six years Shoenfeld's Catalogue, in sixteen years $\frac{8}{10}$ Argelander'sCatalogue.

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The beloved, the excellent Admiral Mouchez in his *Photographic Astronomique a l'Observatoire de Paris et la Carte du Ciel* was right in declaring that, "It will soon be no longer necessary to use great and costly instruments to tire one's health spending nights in maneuvering them, in waiting for favorable circumstances; neither will it be necessary to travel to other hemispheres in order to study certain regions of the heavens, invisible in our own latitudes; all these operations will be transformed in a study done at leisure at the microscope; a study commodious, easy, inexpensive, within the reach of all workers curious of things in Nature. To all of these it is given to penetrate more profoundly into the heavens than can be done to-day (1885) by the majority of professional astronomers."

PARIS, July 25, 1893.

ON THE PERIOD OF THE FIFTH SATELLITE OF JUPITER.*

E. E. BARNARD.

No satisfactory observations of the fifth satellite of Jupiter have been obtained this apparition of the planet until September 15th when it was seen and measured before its eastern elongation. Twenty-one distance measures were made and from these the time of elongation was computed by the formula

$$T = t \pm \frac{1}{0^\circ.502} \cos -1 \frac{\delta}{\Delta}$$

where δ = the measured distance of the satellite, Δ the apparent elongation distance, and t the observed time. The motion of the satellite in one minute being 0°.502.

From these measures the following independent times of elongation (T) were obtained. They are in standard Pacific time (8 hours slow of Greenwich).

$17^h\ 40^m.0$	$17^h\ 38^m.1$	$17^h\ 36^m.4$
17 37 .8	17 35 .0	17 35 .0
17 36 .4	17 36 .5	17 36 .0
17 38 .4	17 35 .3	17 39 .7
17 37 .5	17 33 .6	17 37 .2
17 39 .2	17 38 .0	
17 35 .8	17 34 .8	Mean = $17^h\ 36^m.9$
17 40 .2	17 34 .8	

Combining with this an observation (from measures) of an eastern elongation 1892 Sept. 10 ($12^h\ 47^m.5$), the following period is derived

$$P = 11^h\ 57^m\ 22^s.56$$

* Communicated by the author.

From my observations last year I had obtained, P = $11^h\ 57^m\ 23^s.06$ (A. J. 286).

From the same observations Mr. A. Marth had also derived a period, P = $11^h\ 57^m\ 21^s.88$ (M.N. Vol. LIII, pp. 490-492). It will be seen that the present determination, covering a period of 743 revolutions of the satellite, falls nearly mid-way between Marth's and my previous determination.

It is $0^s.50$ less than mine and $0^s.68$ greater than Marth's.

In the above determination the observations have been corrected for parallax and aberration.

The satellite seems to be about eight minutes behind Marth's ephemeris in the *Monthly Notices* referred to.

The delay in getting observations of this object this year was on account of the elongations falling one too early and the other too late in the night (*i. e.* morning).

At the present observations, though the satellite was faint, it was fairly well seen.

MT. HAMILTON, 1893, Sept. 17.

METEORIC ASTRONOMY II.*

DANIEL KIRKWOOD.

TUTTLE'S COMET AND THE PERSEIDS, OR AUGUST METEORS.

The frequency of shooting stars in August was observed as long since as the middle of the eighteenth century. The fact that a maximum occurs, almost invariably, about the 9th or 10th of the month was announced by Quetelet, of Brussels, in 1835. In clear nights at that time the meteors may always be observed. The numbers are much less considerable than those of November 13th; varying in different years from 20 to 200 per hour. They may be observed in the early part of the night. The November meteors, on the contrary, are rarely seen before midnight. The tracks of the former, when produced backward, intersect in Perseus; hence their designation, *Perseids*.

Meteors observed during August in the ninth, tenth, and more recent centuries, are now known to have been derived from this Perseid cluster. The following catalogue includes all known to the present writer down to A. D. 1819. In later years, only the more important have been selected.

* Communicated by the author.

PERSEID METEORS.

A. D.	811	Cluster A.
	820	
	824	
	830	
	833	
	835	
	841	

A. D.	924	Cluster B.
	925	
	926	
	933	

A. D.	1243	
	1451	
	1709	August 8th
	1779	" 9th-10th
	1781	" 8th
	1784	" 6th-9th
	1798	" 9th
	1799	" 9th-10th

A. D.	1800	August 10th
	1801	" 8th
	1806	" 10th-11th
	1809	" 10th
	1819	" 6th*
	1831	" 10th
	1853	" 10th

The fact that the August meteors move in the orbit of the third comet of 1862 was discovered by Schiaparelli. A comparison of the dates at which they have been observed seemed to indicate 105 years as the meteoric period. The conclusion, however, was very uncertain. The period of the comet, according to Oppolzer, is 121.502 years.† It seems impossible to trace clearly a series of consecutive returns in the preceding list of meteor showers. A close inspection, however, points with considerable probability to the same or nearly the same period—about 122 years. Thus:

A. D.	811 to	A. D.	933.....1	period of 122	years.
	820 to		1798.....8	" of 122	"
	824 to		1801.....8	" of 122	"
	830 to		1806.....8	" of 122	"
	833 to		1809.....8	" of 122	"
	835 to		1811.....8	" of 122	"
	841 to		1451.....5	" of 122	"
	924 to		1779.....7	" of 122	"
	933 to		1789.....7	" of 122	"
	1243 to		1853.....5	" of 122	"
	1451 to		1819.....3	" of 122 +	"
	1709 to		1831.....1	" of 122	"

The facts point to an indefinite antiquity of the cometic and meteoric clusters; to a *slow* change in the relative positions of the principal parts; and to a more considerable disturbance of limited sections.

Tempel's comet of 1866 and Tuttle's meteor comet of 1862 have both retrograde motion. Other well known bodies of this class, as we shall find hereafter, are direct. Their phenomena,

* "During the night of August 6, an aerolite was seen at sea passing from N. E. to S. W. It was preceded and followed by the appearance of a great number of shooting stars."—Quetelet.

† *Annuaire*, 1884, p. 193.

their physical history, the cause and circumstances of their disintegration, etc., etc., will be fruitful themes for the astronomer of the future.

TUTTLE'S COMET—1862 III. THE SOURCE OF THE PERSEID METEORS.

The third comet of 1862 was discovered by Tuttle, at Cambridge, U. S., four days before its perihelion passage. On the 24th it became visible to the naked eye, having then a diameter of 8′, and a tail 1° 30′ in length. On the 27th of August the train was from 27° to 30° in length and the apparent diameter 17′. The comet in 1862 was easily visible to the naked eye, and as its associated meteors have been occasionally seen, widely dispersed, for more than ten centuries, it is highly probable that the comet itself has been a member of the solar system for several thousand years. That the visit of 1862 was the only one at which it was seen seems wholly improbable. Its brilliancy, of course, has depended upon its position with respect to the Earth, but it could hardly have escaped detection at *every* return. What is the testimony of ancient records?

With a period of 121.5 years former returns would have been due about the years named below. Those in which comets appeared are marked by an asterisk. The dates are carried back to the meteoric epoch early in the ninth century.

Returns in A. D.			
1862*	Observed.	1255 or 1254*	Calculated.
1741	Calculated.	1133*	"
1619*	"	1012*	"
1498	"	890*	"
1376*	"		

NOTES.

1. A large fire ball moving in a shower of Perseids, is the phenomenon recorded in Quetetet's catalogue for August, 1819.

2. Recorded facts sustain the following history of the great comet of 1862, and of the thence derived shower of Perseid meteors:

It is generally admitted by astronomers that comets enter the solar system *ab extra*, move in conic sections about the Sun, and if undisturbed by the planets, again pass off from the system to be seen no more. If in their motion, however, they approach very near any of the larger planets, their direction is changed by perturbation—their orbits under certain circumstances, being transformed into ellipses. Such, according to the accepted theory, was the origin of the meteor-comet of 1862. The epoch

of its entrance into the solar system is unknown. The cause and nature of the catastrophe by which a change was effected in its physical constitution are also unknown. At a distant epoch, however, the bonds which united it as a single body were disrupted. The gaseous portion, or that lightly held by the force of gravitation, escaped from the control of the hitherto governing mass, in the form of meteors. The process of disintegration has continued through successive centuries. The orbit intersects that of the Earth in the point passed by the latter about the 10th of August. This cometic matter passing through the atmosphere with great velocity becomes ignited and presents the appearance of luminous meteors. The future of such phenomena cannot be foretold.

3. It is not impossible, or even improbable, that meteor clusters may have been drawn by planetary influence from the track intersecting that of the Earth, so that for a particular section the phenomena of shooting stars may be no longer seen. On the other hand new orbits and periods may result from similar disturbances.

THE ORBIT OF β 416.*

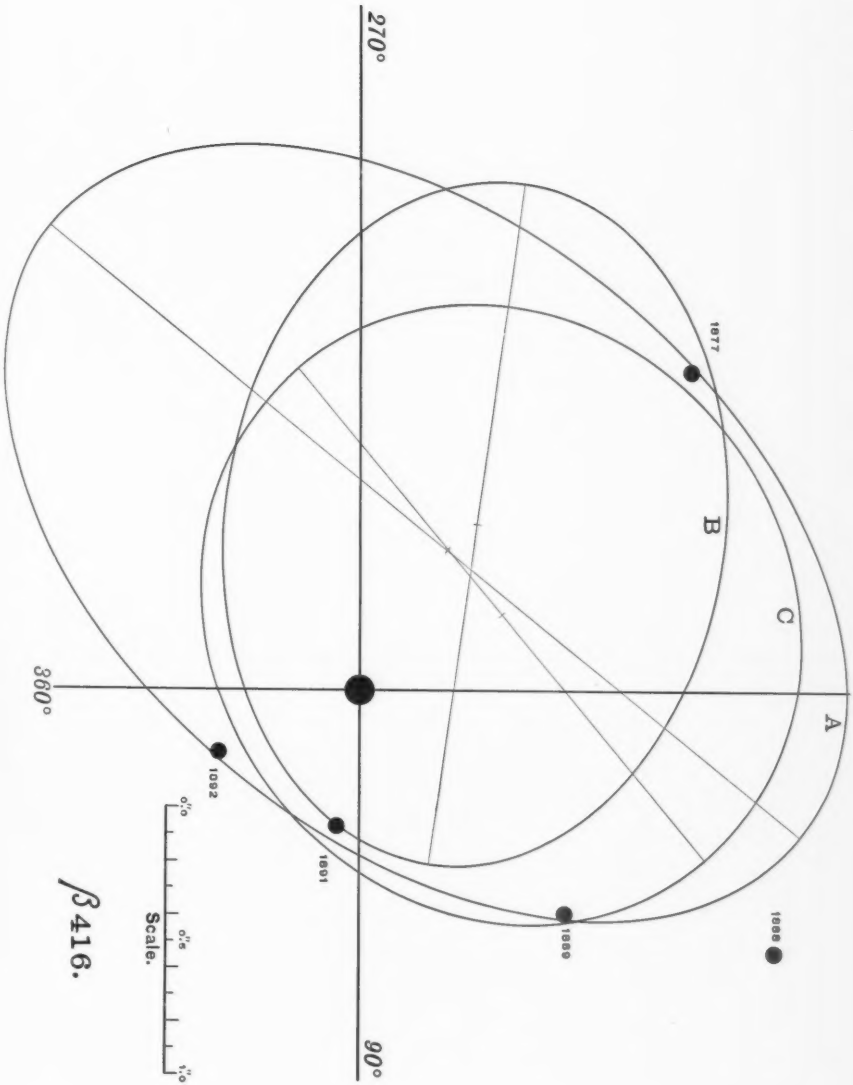
S. W. BURNHAM.

Herschel at the Cape of Good Hope noted a distant companion to the star Scorpii 185, about 30″ from the primary, and entered it as No. 4935 of his catalogue. In 1876, when looking it up with the 6-inch, I found that the large star had a much nearer attendant, the distance being estimated at that time as 1″.8. It was measured in the following year at Cincinnati and Sydney. When it was next examined by me in 1888 at Mt. Hamilton, it was apparent that rapid motion had taken place in the angle. I measured the close pair every year but one during my stayon Mt. Hamilton, and as the distance steadily diminished, the angular movement was correspondingly more rapid from year to year.

After the measures of 1892 were made, I obtained the orbit by the graphical method from all the measures, the angular motion then being about 200°. Since that time two other orbits have been computed, one by Gore (*Monthly Notices*, March, 1893) and the other by Glasenapp (ASTRONOMY AND ASTRO-PHYSICS, May 1893.) These are marked respectively A and B on the accompanying diagram.

* Communicated by the author.

PLATE XLI.



As these several investigations give very different results in the elements of the orbits, and in the apparent ellipses which represent the path of the companion, and as all have used precisely the same data, it will be interesting at this time to examine them as laid down on the same sheet. It will be seen that the measures have been differently treated in the adjustment of the angles and distances. To some extent this may be explained by the methods employed.

The following are the elements of the respective orbits:

	Gore.	Glasenapp.	Burnham.
	A	*B*	*C*
P	= 3448 years	34.85 years	24.7 years
T	= 1891.85	1892.00	1892.26
e	= 0.556	0.65	0.56
a	= 2″.13	1″.52	1″.46
i	= 56°.7	45°.4	44°.4
Ω	= 139°.4	104°.3	122 .0
λ	= 278°.2	300° 7	93 .5

At this time is is impossible to say which of these ellipses will best represent the orbit, but it is certain that the measures of this year and the next year will substantially determine the question. I examined this star during the present season with the 18½-inch of the Dearborn Observatory, but in this latitude the star is so near the horizon I could do no more than see that it was apparently double. So far as one could judge, the distance had not sensibly increased since my measures in 1892.

The position of this pair (1880) is:

R. A. $17^h\ 10^m\ 47^s$
Decl. $-34°\ 51'$

I have previously called attention (*Sidereal Messenger*, December, 1891) to the very rapid motion of this system, considering the distance of the components, and suggested that it may be comparatively near our system. It would be well for some observer in the southern hemisphere to investigate the parallax of this star. The Herschel companion is admirably well placed for this purpose.

CHICAGO, Sept. 1, 1893.

THE ORBITS OF COMET 1889 V.*

H. C. WILSON.

This comet was discovered by Mr. W. R. Brooks, then of Phelps, N. Y., July 6, 1889. It was a rather faint telescopic object and would not have attracted very much attention, but for the fact

* Communicated by the author.

that it was moving in a short ellipse with a period of about 7 years, and that in the year 1886 it must have passed very close to the planet Jupiter and its orbit must then have been very greatly changed. This was pointed out by Mr. S. C. Chandler, in No. 204 of the *Astronomical Journal.* In a later paper (*Astr. Jour.* No. 205), Mr. Chandler gave the results of a rough calculation of the principal perturbations by Jupiter, that is from Jan. 24 to Sept. 14, 1886, and attempted to trace the course of the comet backward from that time. He found that the encounter with Jupiter in 1886 effected a complete transformation of the comet's orbit. Instead of the present small seven-year ellipse, it was previously moving in a large one of about 27 years period, whose aphelion lay outside of Saturn's orbit, and whose perihelion was almost exactly at the present aphelion distance. The direction of the lines of apsides and nodes were reversed and turned through an angle of about twenty degrees. The plane of the orbit was also tilted about fourteen degrees.

Furthermore, tracing back the course of the comet with the elements of the twenty-seven year ellipse Mr. Chandler found that the comet must have been very near Jupiter in the year 1779, the very time when the lost comet of Lexell 1770 was in the immediate vicinity of the planet and suffered the notable disturbance which was supposed to have taken it out of our reach. This coincidence of time and place afforded a strong presumption of the identity of the two comets. Later computations do not confirm this presumption but there remains the very interesting and difficult problem of determining exactly the course of a comet when under the preponderating influence of a great planet.

In recent numbers (302 and 303) of the *Astronomical Journal*, Mr. C. L. Poor has given the results of his investigations in regard to Comet 1889 V. He used as the basis of his work the definitive elements of the comet's orbit computed by Dr. Julius Bauschinger from all the published observations, the latter extending over a period of over eight months. There is still some uncertainty expressed by the factor ν, in these elements, not due to the computations but to the inaccuracies of the observations. Mr. Poor finds that ν is probably within the limits -40 and $+40$. This uncertainty is very slight but enough to affect, to some extent, the character of the calculated approach of the comet to Jupiter. Taking the most probable values of the elements and calculating the perturbations by all of the planets, which could preceptibly influence the comet's motion, back to 1886, Mr. Poor finds that the approach was closer than Mr. Chandler had supposed. The nucleus of the comet passed within

the orbit of the first satellite and may have almost grazed the surface of the planet itself.

In calculating the path of the comet when very near to Jupiter Mr. Poor followed the method of transforming the elements of the elliptic around the Sun into those of a hyperbola around Jupiter, regarding the Sun as a disturbing body. This transformation was made at the date Oct. 26.5, 1886. After tracing the movement of the comet back around Jupiter for six months the elements were again referred to the Sun as a center at the date March, 24.5, 1886, and it was found that the orbit had so greatly changed that the period was then 41.87 years. For several months before that time, however, the perturbations by Jupiter had been large and when these were calculated back to March 14.5, 1884, the elements were found to give a period of 31.38 years, with the perihelion passage July 20, 1886. Mr. Poor thinks that the uncertainty of this period is within two years.

It is interesting to place side by side the elements of the comet's orbit at the various stages of its transformation. We can give here only the elements obtained on the supposition that $\nu = 0$. Mr. Poor gives seven sets of elements corresponding to different values of the factor of uncertainty ν.

ELLIPTIC ORBIT ABOUT SUN.

	1869 Sept. 30.5 Berlin M. T.		1886 Oct. 26.5 Gr. M. T.	
$M =$	0° 01′ 05″.01 + 1″.0ν		213° 15′ 46″.82	
$\pi =$	1 34 54 .99 − 3 .0767ν		2 35 23 .59	
$\Omega =$	17 59 04 .37 − 0 .0174ν	1890.0	19 02 59 .88	1886.0
$\omega =$	343 35 50 .62 − 3 .0593ν		343 32 23 .71	
$i =$	6 04 06 .57 − 0 .1140ν		7 27 03 .80	
$\varphi =$	28 05 05 .75 − 1 .3961ν		31 48 33 .33	
$\mu =$	501″.72306 + 0″.0114ν		522″.09375	
$a =$	3.684350		3.587900	

HYPERBOLIC ORBIT ABOUT JUPITER.

	1886, Oct. 26.5 Gr. M. T.	1886, March 24.5
$\pi =$	283° 41′ 55″.12 + 249″.59ν	284° 13′ 39″
$\Omega =$	258 10 11 .80 − 14 .21ν	256 08 56
$i =$	74 52 08 .00 − 120 .66ν	63 42 15
$F =$	76 34 22 .82 + 19 .30ν	−77 38 50
$a =$	−0.0856771 + 0.000010274ν	−0.0884240
$e =$	1.0097088 + 0.000179192ν	1.011393

ELLIPTIC ORBIT ABOUT SUN.

	1886, March 24.5		1884, March 14.5	
$L =$			161° 49′ 54″	
π	187° 14′ 00″.7		188 46 12	
Ω	183 17 16 .8	1886.0	186 19 14	1886.0
i	7 39 17 .6		6 45 44	
ω	3 56 43 .9		2 26 58	
φ	33 12 05 .4		26 47 33	
μ	84″.7435		113″.0620	
a	12 .05776		9 .94293	
T	1886, June 24.0752		1886, July 20.2416	

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For the benefit of those who do not readily interpret the elements we have attempted to represent them graphically by means of the diagrams, Figures 1 and 2. In Fig. 1, the four elliptic orbits are shown together with the orbits of Earth, Jupiter, Saturn and Uranus. The four ellipses of the comet's path all meet near that point of Jupiter's orbit which is opposite the vernal equinox. For two of them this is the most distant part of the orbit (aphelion) for the other two it is the nearest part (perihelion). One can see at a glance what a tremendous change in the path of the comet occured between March and October, 1886.

Interpreting the elements with the aid of the figure they tell us that the comet previous to 1884 was moving in a large ellipse, the nearest point of which to the Sun lay very close to Jupiter's path, and the farthest point about half way between the orbits of Saturn and Uranus. The plane of the orbit was inclined at an angle of about 7° to ecliptic and the orbit crossed the latter from south to north in longitude 186°, very near the path of Jupiter. This orbit is represented in Fig. 1 by the line marked "Orbit of Comet 1889 V in March, 1884." The comet was describing this path at the rate of a revolution in about 31.38 years. Had it not been influenced by Jupiter it would have gone around this orbit again and again and never have been seen from the Earth. As it happened to come to perihelion at the same time that Jupiter was in that vicinity, its was drawn, at first very gradually, then more rapidly, from the large smooth curve of figure 1 into the larger dotted curve, then into the smaller dotted curve, and finally and gradually into the slightly larger smooth curve. This last curve, "Orbit of Comet 1889 V in 1889," it will continue to follow for several revolutions of seven years period with only slight modifications by the planets. In 1921 another close approach to Jupiter will occur, which, though not so close as the one in 1886, will considerably change the orbit, probably making it larger and possibly removing the comet from the reach of our telescopes for a long period.

Fig. 2 represents a portion of the hyperbolic orbit of the comet relative to Jupiter while it was passing through the system of the satellites, and is especially interesting in showing how very close the comet came to the planet and the satellites. The scale is nearly 4000 times that of Fig. 1 The time of nearest approach to Jupiter was found to be July 20 when the distance was only 2.31 radii of the planet, with the uncertainty expressed by ν in the formula

$$q = 2.31 + 0.033\nu$$

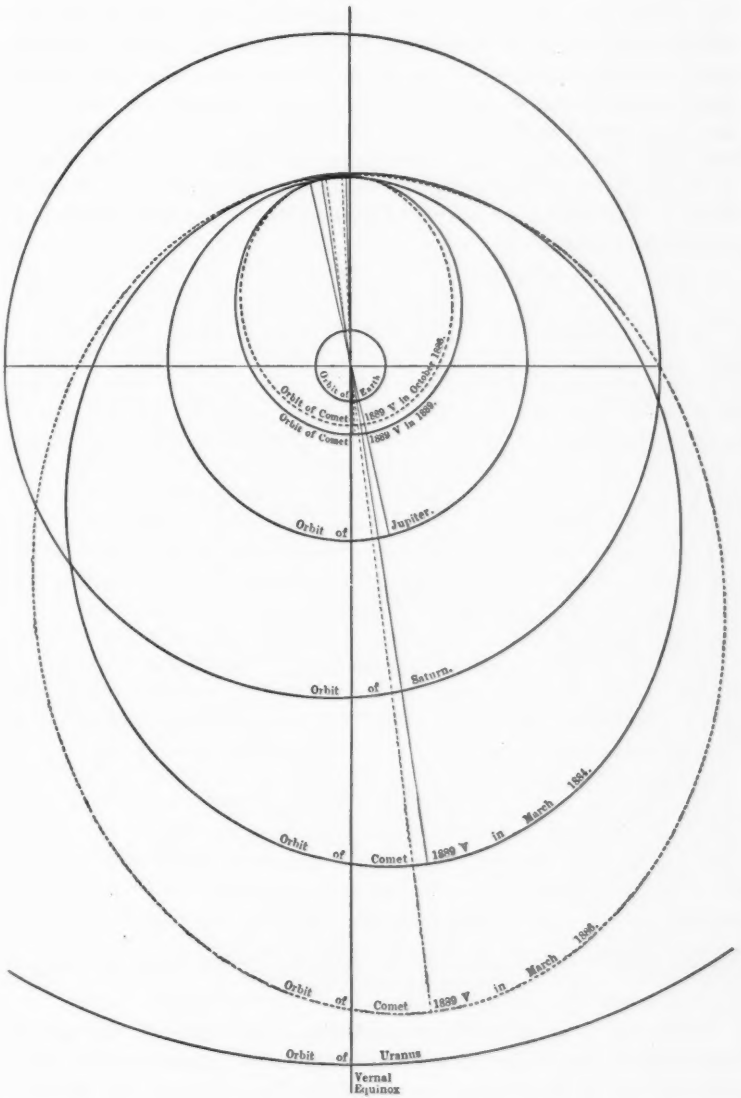
Orbit of Earth.
Orbit of Comet 1889 V in October 1886.
Orbit of Comet 1889 V in 1886.
Orbit of Jupiter.
Orbit of Saturn.
Orbit of Comet 1889 V in March 1884.
Orbit of Comet 1889 V in March 1886.
Orbit of Uranus
Vernal Equinox

FIG. 1.—THE ORBITS OF COMET 1889 V BEFORE AND AFTER ITS APPROACH TO JUPITER IN 1886.

"That is, the comet not only passed through the system of Jupiter's satellites, but it actually passed within the orbit of the first satellite whose mean distance is 5.93 radii of the planet. Taking the extreme limits of ν we are safe in saying that the comet passed the center of Jupiter at a distance not greater than 3.63 and not less than 1.00 radii of the planet. In other words the center of the comet may have grazed the surface of Jupiter, and it certainly approached that surface to within a distance of 2.63 radii of the planet, or only 112,300 miles. Even this latter is a very small quantity.

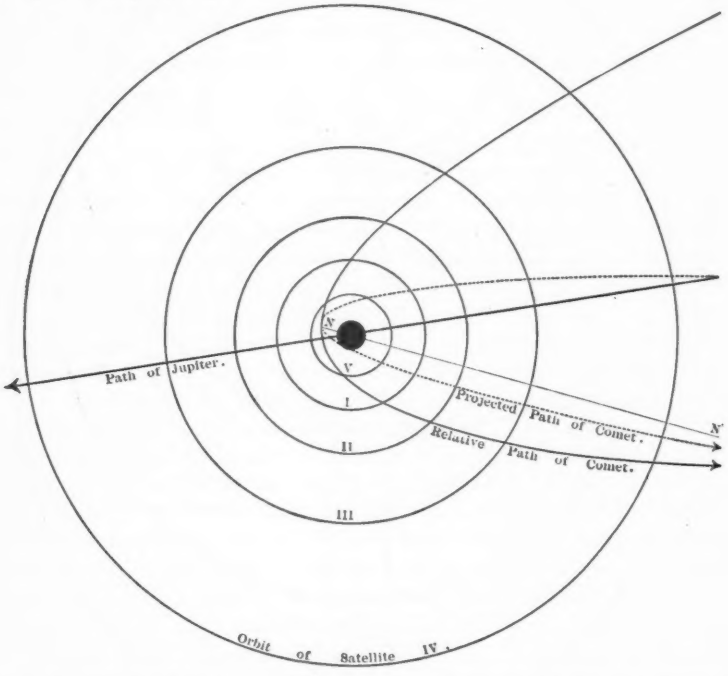

FIG. 2.—THE PATH OF COMET 1889 V THROUGH JUPITER'S SATELLITE SYSTEM JULY 18 TO 21, 1886.

"For the most probable hypothesis, that of $\nu = 0$, the comet was 2.65 days within the system of Jupiter's satellites, and during this time it made nearly a complete circuit about the planet, passing over an arc of 313° of longitude. The comet entered the Jovian system in longitude 118° on July 18.77, passed the planet July 20.10, at a distance of only 2.28 radii, and July 21.43 left

the system in longitude 71°. During this time it must have collided with one or more of the satellites."

The last two paragraphs are quoted from Mr. Poor's paper. In Fig. 2 the circles represent the orbits of Jupiter's satellites and the relative path of the comet is almost a parabola and was drawn from the mean of the hyperbolic elements given for Oct. 26.5 and March 24.5, 1886. It is necessary to understand that the path of the comet is in a plane inclined at an angle of about 70° to the plane of the satellite orbits, so that the comet did not pass horizontally across them, but came up through from below. The line NN′ represents the line of nodes or intersection of the planes of the comet's path and the ecliptic.

The reader must understand, too, that this is only the path relative to Jupiter and that the latter was at the same time moving rapidly along its orbit, so that while the comet, after July 20, was apparently moving backward with reference to Jupiter, with reference to the Sun it was moving forward and changing from the larger dotted orbit to the smaller one in Fig. 1.

It will be noticed that the path of the comet passes very close to that of the new satellite (V) of Jupiter and suggestions have already reached us from different sources, that possibly this comet had something to do with the origin of the new satellite, that in fact the satellite is a captured fragment of the comet. We cannot see how this could be, with the comet passing through the system just as it did, and, supposing the capture to have occurred, cannot account for its brilliancy, since the whole comet was invisible long before it reached the distance of Jupiter.

If the period of 31.38 years is correct this comet cannot be identical with Lexell's comet of 1770 unless marked perturbations occurred between 1886 and 1779 when the latter was in the vicinity of Jupiter and had its orbit greatly changed, for the interval 107 years is not a multiple of 31.38 years or any number very near that. Mr. Poor finds that there was no very near approach of the comet to Saturn in that interval but that possibly a very close approach to Jupiter occurred in 1791. This however, would be fatal to identity with Lexell's comet, for it would require the comet between 1779 and 1791 to have a period equal to that of Jupiter, that is an identical orbit.

Mr. Poor also tests the question by means of M. Tisserand's formula

$$n = \frac{1}{a} + \frac{2\sqrt{A}}{R^2}\sqrt{p}\cos i$$

and finds that for the four sets of elliptic elements of Comet 1889 V, at the epochs March, 1884, March, 1886, Oct., 1886, and Sept., 1889, the values of n agree, while that from the elements of Lexell's comet is decidedly different. The question of the disruption of the comet and the possibility that a portion of it was drawn permanently into Jupiter's system he proposes to discuss in another paper.

THE JUPITER FAMILY OF COMETS.*

W. W. PAYNE

Some little time ago we had occasion to look up the comets supposed to belong to the family of Jupiter in connection with the study of the Holmes' comet, a recent member of the notable group. Thinking that an illustration of the several paths of these comets in connection with the orbit of Jupiter would be interesting to the readers of *Popular Astronomy*, they were projected carefully, and very nicely engraved as the accompanying plate shows. Not knowing of any illustration that is nearly so complete as this, we ventured to give it place for convenience in reference. We do not feel sure that all the comets found in the following table rightly belong to this family, neither are we very certain that there are not others which might as well be reckoned in the list as some which find place there. At all events the diagram and the table of elements are arranged and put in this form after much pains in search for best and most reliable results.

Another point of interest is that which is spoken of by most late writers who refer to such cometary groups with much particularity, and that is the tendency of the perihelion points of the orbits to cluster in one region lying in the general direction of the vernal equinox. This seems to be manifestly the case in the Jupiter family. Why this is so, we are not aware that authors offer any very satisfactory reason. The only one that has occurred to us depends on the "capture theory," which is itself not generally accepted by astronomers as proved, but which has, at least, a good degree of probability. The "Sun's way," so-called, is nearly in the direction indicated by the bottom of the chart. Jupiter's absolute motion in the region of the autumnal equinox must equal his mean motion plus that of the "Sun's way," approximately. Jupiter therefore would overtake or meet more comets in that part of his orbit than in others, and so the possibility of disturbing influence in that region would be greater than elsewhere. This may not be new, but we have not seen the statement before. The chart is taken from *Popular Astronomy*.

* Communicated by the author.

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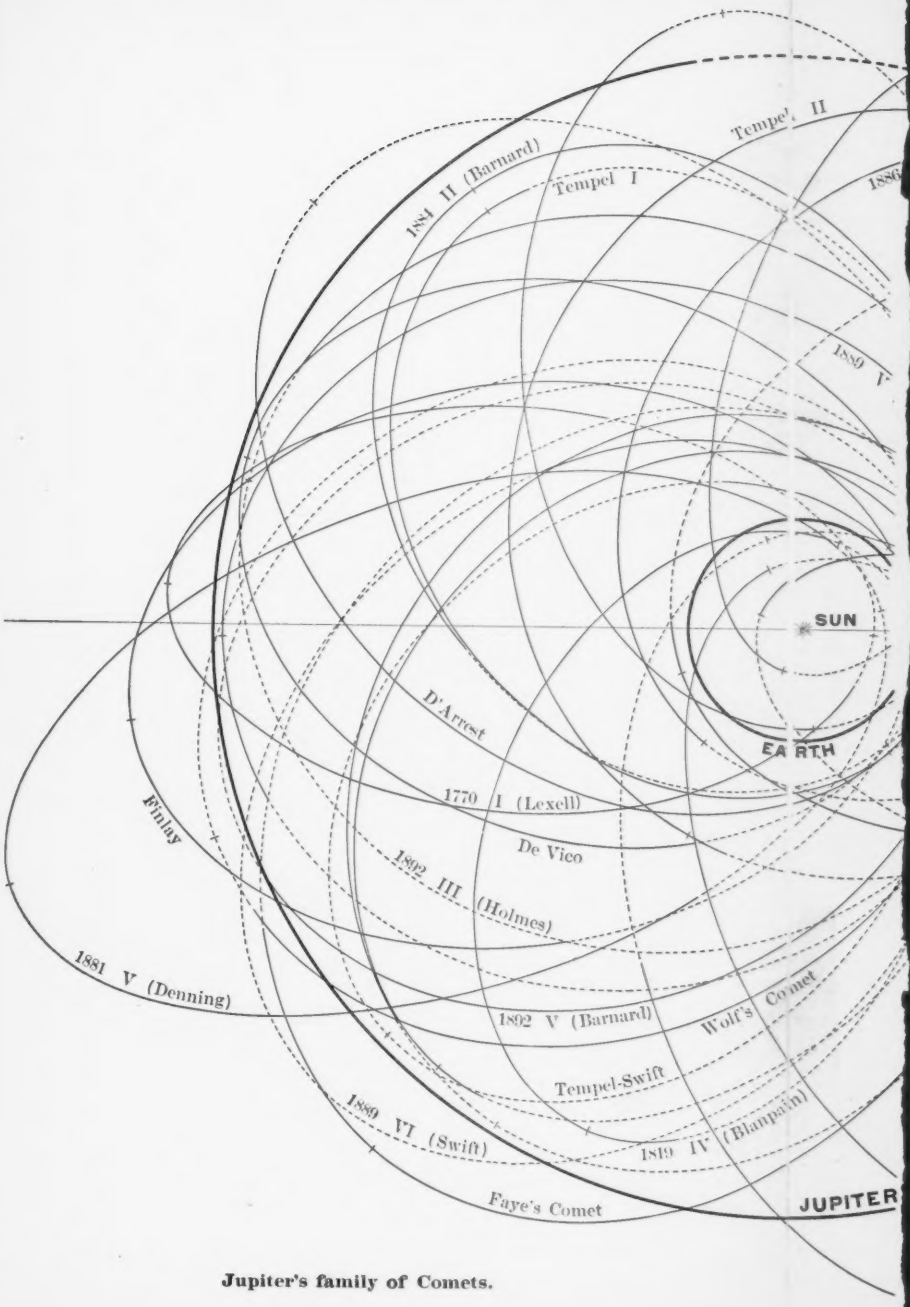

Jupiter's family of Comets.

From a drawing by A. G. Sivaslian,
Northfield, Minn.

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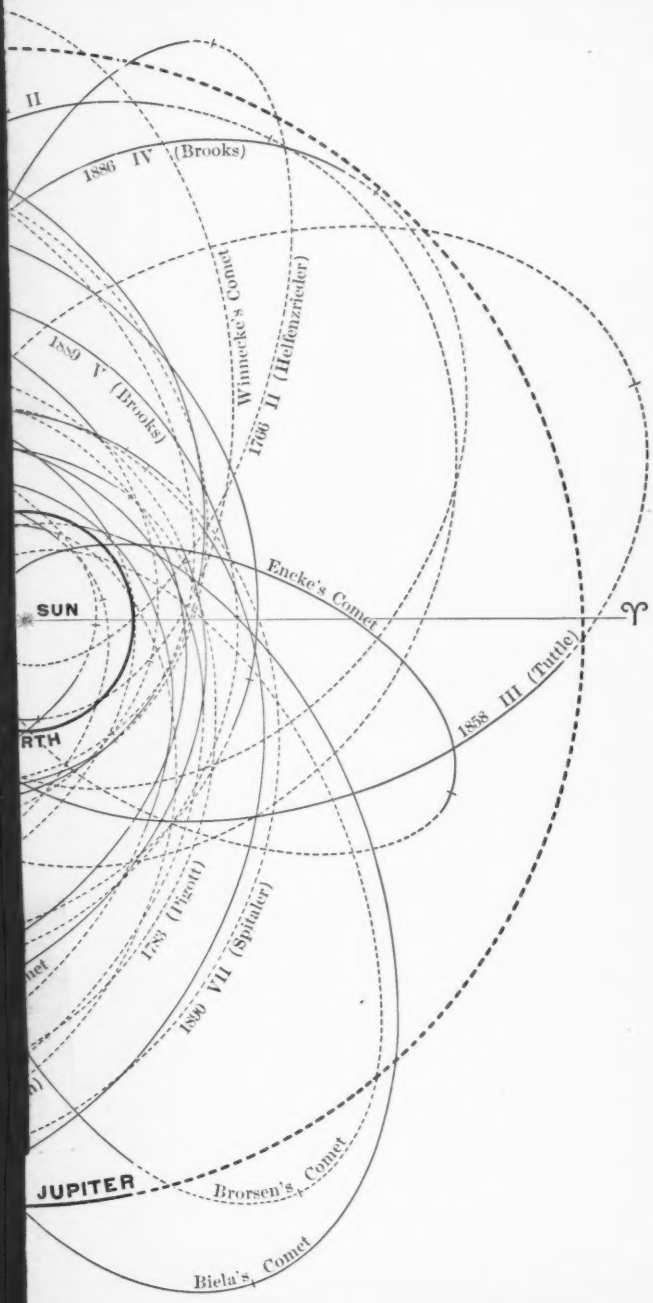

Goodsell Observatory,
Carleton College.

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ELEMENTS OF THE JUPITER FAMILY OF PERIODIC COMETS.

Comets of which more than one apparition has been observed.

Name.	Period in yrs.	Time of Perihelion Passage.	Perihel'n Distance	Aphelion Distance	e	π	Ω	i	Mean Equinox	Calculator and Reference.
						° ′ ″	° ′ ″	° ′ ″		
Encke	3.303	1891 Oct. 17.986	0.34047	4.09489	0.84647	158 38 46	334 41 27	12 54 58	1891.0	Backlund, Astr. Gesel. J. 27-1
Tempel	5.211	1889 Feb. 2.101	1.38660	4.66545	0.55210	306 08 03	121 09 17	12 45 05	1890.0	Schulhof.
Brorsen	5.456	1890 Feb. 4.104	0.58776	5.61038	0.81034	116 23 10	101 27 34	29 23 48	1890.0	E. Lamp, A. N. 2933.
Tempel-Swift	5.534	1891 Nov. 14.083	1.08660	5.17088	0.65270	43 14 16	296 31 15	5 23 14	1891.0	Bossert, Astr. Gesel. J. 27-1.
Winnecke	5.818	1892 June 30.893	0.88642	5.58313	0.72297	276 11 09	104 04 59	14 31 31	1890.0	Haerdtl. A. N. 3062. Astr. Gesel. J. 28, p. 142
Tempel	6.507	1885 Sept 25.734	2.07332	4.89733	0.40513	241 21 50	72 24 09	10 50 27	1880.0	Gautier, A. N. 2656.
Biela { Nucleus 1	6.587	1852 Sept 23.718	0.86016	6.16732	0.75520	109 05 20	245 49 34	12 33 28	1852.0	D'Arrest, A. N. 933
Biela { Nucleus 2	6.629	1852 Sept 22.952	0.86059	6.19687	0.75512	108 58 17	245 58 29	12 33 50		
Finlay	6.627	1893 July 12.176	0.98912	6.06371	0.71951	7 41 34	52 27 43	3 02 02	1893.0	Schulhof, A. N. 3171
D'Arrest	6.691	1890 Sept 17.493	1.32404	5.77776	0.62713	319 14 34	146 16 32	15 42 41	1890.0	Leveau, Astr. Gesel. J. 26-1
Wolf	6.821	1891 Sept 3.473	1.59285	5.60058	0.55714	19 11 38	206 22 29	25 14 33	1891.0	Thraen, A.N. 3050, Berberick, A. J. 253, Astr. Gesel. J. 27-1
Faye	7.566	1881 Jan. 22.671	1.73814	5.97009	0.54902	50 48 47	209 35 25	11 19 40	1880.0	Möller, Berl. Jahrb. 1882

Comets of which only one Apparition has been Observed.

1819 IV, (Blanpain)	4.810	1819 Nov. 20.252	0.89256	4.806	0.68675	67 18 48	77 13 57	9 01 16	1819	Encke.
1766 II, (Helfenzrieder)	5.025	1766 Apr. 26.995	0.39898	5.468	0.86400	251 13 00	74 11 00	8 01 45	1766	Burckhardt.
1884 II, (Barnard)	5.398	1884 Aug. 16.483	1.27969	4.872	0.58395	306 11 20	5 08 38	5 27 33	1884.0	Egbert, A. N. 2657
1844 I, (De Vico)	5.459	1844 Sept 2.484	1.18632	5.015	0.61737	342 30 48	63 49 38	2 54 46	1844	Brünnow.
1886 IV, (Brooks)	5.595	1886 June 6.691	1.32772	4.976	0.57874	230 16 51	53 28 57	12 43 26	1886.0	S. Oppenheim.
1770 I, (Lexell)	5.626	1770 Aug. 13.547	0.67431	5.652	0.78684	356 16 27	131 59 34	1 34 31	1770	Le Verrier.
1783 (Pigott)	5.888	1783 Nov. 19.937	1.45929	5.062	0.55246	50 17 25	55 40 30	45 06 54	1783	C. H. F. Peters.
1892 V, (Barnard)	6.309	1892 Dec. 11.05	1.42911	5.396	0.58123	16 52 36	206 38 45	31 12 28	1892.0	Krueger, Astr. Gesel. J. 28 p. 145
1890 VII, (Spitaler)	6.378	1890 Oct. 26.601	1.81791	5.061	0.47144	58 25 58	45 05 52	12 50 44	1890.0	Spitaler, A. N. 3011
1858 III, (Tuttle)	6.609	1858 May 2.974	1.14922	5.894	0.67368	200 46 27	175 04 09	19 30 02	1858.0	Schulhof.
1892 III, (Holmes)	6.909	1892 June 13.238	2.13940	5.116	0.41024	345 53 12	331 42 12	20 47 23	1892.0	Schulhof, A. N. 3140
1889 V, (Brooks)	7.073	1889 Sept 30.012	1.95023	5.419	0.47070	1 26 17	17 58 45	6 04 10	1890.0	Chandler, A. J. 205 A. N. 2952 Astr. Gesel. J. 26 1
1881 V, (Denning)	8.343	1881 Sept 12.834	0.72384	7.503	0.82403	18 10 05	66 09 02	6 53 26	1881.0	Chandler, A. N. 2406
1889 VI, (Swift)	8.534	1889 Nov. 29.572	1.35367	6.998	0.67585	40 15 02	330 36 02	10 14 54	1890.0	Hind, Astr. Gesel. J. 27-1

Astro-Physics.

ON THE SPECTRA OF THE ELEMENTS.*

H. KAYSER AND C. RUNGE.

Our researches have, as we think, conclusively shown that at least in the spectra of a number of elements the distribution of the lines is subject to certain laws:

1. A considerable number of the lines of each spectrum, in many cases the majority, may be arranged in series that resemble the one in the hydrogen spectrum discovered by Huggins. The wave-lengths of the lines forming a series may be calculated by a formula similar to the formula discovered by Balmer for the series of hydrogen lines. If λ be written for the wave-lengths we find

$$\lambda^{-1} = A - Bn^{-2} - Cn^{-4}$$

where A, B, C are positive constants and for n the series of entire numbers beginning with 3 is to be substituted. This formula we first published at the meeting of the British Association at Bath, 1888 (see Rep. of Brit. Ass., p. 576, 1888). Since then we have been occupied with determining the spectra of the elements, as the new methods and instruments of Rowland were so superior as to make the existing determinations of wave-lengths almost worthless. We began by measuring the spectrum of iron, which has served as a scale, and after that the spectrum of the carbon flutings, before we entered on the investigation of the spectra of theelements.

2. All series except those in the spectrum of hydrogen and of lithium may be said to consist of either doublets or triplets of lines. Among those consisting of doublets two different characters appear, series whose doublets are more and more narrow toward the more refrangible side, and series whose doublets retain the same difference of oscillation-frequencies throughout. The former kind has only been observed in the spectra of the alkalis, where there are in each spectrum two series of the latter and one series of the former character. The different character may also be stated thus. For the first kind A and B have the same values in the two formulas that give the first and the second lines of the doublets, while for the second kind B and C have the same values. In a drawing of the spectrum to the scale

* Read at the Congress of Astronomy and Astro-Physics, Chicago, August, 1893.

of oscillation frequencies the figure representing the series of the first lines could therefore, for series of the second character, if shifted to the side, be made to coincide with the series of the second lines. In all series of triplets that have been observed the oscillation frequencies of the three lines forming a triplet give the same differences throughout the whole series, which may also be stated thus: The three formulas representing the first, the second and the third lines have the same value for B and the same for C.

3. The value of B is nearly the same in all formulas.

4. If the elements are grouped according to the natural system of Mendelejeff, their chemical relationship is clearly shown in the distribution of the series, which in each family appear to be arranged after a certain plan. For an element of higher atomic weight, the series shift toward the less refrangible side of the spectrum. Thus the spectra of the alkalis show a common plan. So do copper and silver (in the spectrum of gold we have not been able to discover any series). Magnesium, calcium, and strontium show two series of triplets each, arranged correspondingly (in the spectrum of strontium only one was discovered) and so do zinc, cadmium, and mercury. Again aluminium, indium, and thallium show two series of doublets each, also arranged according to a common plan.

5. In each group of chemically related elements, the doublets or triplets, as the case may be, widen with increasing atomic weight. In the spectra of the alkalis the difference of oscillation frequencies is almost exactly proportional to the square of the atomic weight. In the other groups the deviation from this law is greater.

6. The number of lines that do not enter into the series is small for the elements of low melting point and becomes considerable only for the elements of high melting point.

7. The groups of elements investigated belong to the three first columns of the natural system of Mendelejeff. It appears that as a whole from column to column the series shift to the more refrangible side with increasing atomic weight. For this reason we think it possible that in the further columns the series are situated in the most refrangible part of the spectrum and perhaps can not be photographed on ordinary plates, which, as is well known, lose their sensitiveness at about 1900 A. U.

We think that our results are also of some consequence for astro-physics. It is in many cases difficult to decide, whether a certain element occurs in the Sun or in a star. If, for instance, a stronger line of the element is wanting in the spectrum of the

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star, while a weaker one is observed, it is not safe to conclude that the observed line mustbe an accidental coincidence. For the spectrum of the element in the star may differ widely from the spectrum of the same element in our laboratories, just as much as the arc spectrum and spark spectrum of an element differ. But the argument would be correct if applied to lines of the same series. For it seems certain that lines of the same series correspond to oscillations of the same sort of molecule, so that a weaker line of the series would not appear without the stronger one.

A good many of our results have been anticipated by Rydberg, who published an abstract of his researches as early as 1890, a short time before the publication of our researches on the spectra of the alkali metals. In his elaborate work, however, he refers to our memoir.

ELECTRO-MAGNETIC THEORY OF THE SUN'S CORONA.*

HERMANN EBERT.

Many attempts have been made to establish a connection between the optical phenomena of the Sun perceived through the telescope or fixed on the sensitive plate, and the electrical disturbances which must take place in the Sun or result from the magnetic influences exerted by it. The necessary physical experiments have, however, as I believe, only been attained in later times by the series of experiments and theoretical investigations on electro-magnetic radiation commenced by Hertz. Indeed, we gain by these experiments and by the electro-magnetic theory of light well established by them, explanations of many phenomena of solar physics, otherwise obscure. As an example of this, I wish in the following to sketch an explanation of the Sun's corona from this point of view.

For this purpose there are two points chiefly to be made intelligible:

(1) The filamentary structure of the corona.

(2) Its connection with the period of activity of the Sun, and its zones of activity, for we notice that the corona is widely developed and of streaming radiance at the time of the Sun-spot maximum, that on the contrary it appears small, unimportant,

* Read at the Congress of Astronomy and Astro-Physics, Chicago, August, 1893.

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hazy, and without distinct rays, at the time of the minimum; that its strongest rays principally correspond to the region of the spots, etc.

For the explanation we start from the two following premises, the validity of which has never been brought into question, and which form the basis of nearly all solar theories:

(*a*) The regions, where the corona is seen at total eclipses, are not empty, but are completely filled with matter. It has even been said that the corona is really the atmosphere of the Sun. But this does not seem quite correct, because one cannot distinguish in the Sun between the nucleus and the atmospheric envelope as in a planet; according to the solar theory of Mr. August Schmidt, which seems to have much in its favor, that which we call the Sun's surface is only an optical phenomenon caused by total reflections in the fiery masses of gas. Still the gases perceived in the vicinity of this apparent surface by their spectrum, such as hydrogen, helium the D_3 substance, etc., may expand into the real coronal region, if only in the highest degree of rarity. Further there can be no doubt, that in the neighborhood of the Sun, meteoric dust always exists, sometimes collected into clouds; besides this, comets occasionally pass through these regions. On the whole it may be said that to the furthest boundary filled with coronal rays we must take for granted the existence of matter. But this matter is like all other, capable of dielectric polarization; when electric forces are excited in the vicinity of the Sun, all this matter is subject to dielectric polarization.

(*b*) We assume in the second place, that the Sun is the seat of electrical, and therefore also of magnetic forces, for every change in the electric condition of a system is necessarily connected with the appearance of magnetic forces also, and *vice versa*, as appears most clearly in Maxwell's theory.

The Sun is therefore the seat of *electro-magnetic* disturbances.

Electric forces diverging from the Sun were assumed by Fr. Zöllner as explaining the tails of comets and later on in many other theories; they have also been used already in explanation of the corona, but it was imagined that glowing material particles were rent asunder from the Sun's mass and driven forward by electric repulsive forces. However, with this explanation of the coronal rays, we come into collision with dynamical principles.

It was not then known that rapidly changing dielectric polarizations spread out in streams sufficed for this purpose.

In addition to this very simple assumption never called in question in this comprehensive sense, we bring to our aid the two following experimental facts:

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(*A*) Theoretical discussions by J. J. Thompson, and experiments with conducting spheres by Oliver Lodge have shown, that when we produce in any manner, disturbances of electrical equilibrium, these complete themselves in the form of oscillations. Such oscillations are communicated to the surroundings, accordto H. Hertz, and migrate from point to point as a wave of electro magnetic radiation with the velocity of light.

The period of the normal oscillations of the whole Sun is 6½ seconds, corresponding to a wave 120,000 miles long in inter stellar space. Generally these disturbances do not affect the entire Sun, but only a portion, and indeed those parts will experience the strongest electric disturbances which also show violent excitement in the telescope or on the photographic plate by the close connection between the electro-magnetic and the heat and light radiation.

(*B*) Another experimental support was afforded by researches conducted during the last two years by Professor E. Wiedemann and myself.* It was found that all rarefied matter, for example, a gas in an electrodeless vacuum tube, or a vapor of low tension, becomes luminous under the influence of very rapidly alternating electric stresses *at those points where the energy varies most.* If therefore, from a conducting surface, dielectric stresses emanate, but *not* with the same density in all directions, then we perceive a luminosity around the surface, not a homogeneous one, but one of definite structure.

With regard to this we come to the following conclusion: The corona is the visible reaction of the finely divided matter in the vicinity of the Sun, upon the dielectric polarizations proceeding from the different parts of the Sun.

That phenomena like the corona are really produced under these circumstances may be shown by simple experiments. It is only necessary to excite, on a conducting ball mounted in a rarefied atmosphere, such periodically changing electric oscillations of slow damp. Then bright radiating streamers will be seen coming out of the ball. To produce the most suitable oscillations I made use of the following arrangement:

The poles of a large influence machine of twenty couples were joined with two plate condensers, the first plates of which discharged themselves through a spark-gap about 300 times a second. The two other plates were connected by two parallel

* We were aided in these experiments by the Elizabeth Thompson Fund in Boston, and we embrace this opportunity of thanking the administrators of the fund.

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straight wires 4 metres long with a third condenser; on the parallel wires a metallic joint, the so-called "bridge," was pushed to and fro until there was an exact resonance between the two parts of the conducting system.

This is Lecher's arrangement, excellent on account of its symmetry, completeness and the long duration of its oscillations.

With one plate of the third condenser a brass ball 1.5 cm. in diameter was connected by a long wire. The ball was fastened and insulated in the centre of a large air-tight glass cylinder (20 cm. in diameter and 28 long) closed at one end by a thick plane parallel glass plate. The cylinder could be filled with different gases and evacuated. When the ball was charged and discharged, during the play of the sparks in the gap, radiating streams came out of it without any conductor on the external surface of the glass cylinder.*

The streams go out from the points of the ball, on which the causes disturbing the electric conditions of the surroundings are most closely massed together, particularly on irregular curved parts of the surface, caused in our experiments by small variations from a true sphere. The phenomena produced in this manner, which I have photographed in some cases, show the following special qualities, and find their perfect analogies in the Sun's corona:

(α) The ball is surrounded by an aureole of light, through which rays shoot in all directions starting from the surface; from this the aureole derives a special structure.

(β) The aureole is very expanded and the ray-like structure comes out very distinctly with strong electric excitement, *i. e.*, with long spark-gap, accordance with the corona's development at the time of the maximum of the Sun's activity, or over the regions of increased activity, *i. e.*, over the spot zones. It is specially observable that sometimes very bright radial streamers shoot out far over the mean limits of the aureole exactly as they do in greatly developed coronas.

(γ) The aureole is small and insignificant, but above all, hazy and structureless with slighter electric disturbances, *i. e.*, in our case with short spark-gaps; this corresponds with the behavior of the corona at the times and regions of the minimum of the Sun's activity.

(δ) There often shoot out of the ball, especially under higher

* Such a conductor joined with the earth, or the other plate of the condenser was used in our former experiments, and later by Mr. M. I. Pupin (*Amer. Jour. Sci.* (3), 43, p. 463, 1892).

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pressure, very long rays in all directions, rays of 12 cm. in lentgh and more, exceeding therefore the ball's diameter eight times and more; they do not always proceed radially from the surface, they even occasionally seem, on account of the perspective, to leave the ball at the tangent. These streams are formed chiefly when the outside wall of the glass vessel is covered with a conducting metallic sheet connected with the earth. This is analogous to the case where widely spread out masses of cosmic dust are in the Sun's vicinity; this is capable of receiving a great deal of energy, because it has greater capability of dielectric polarization than the free ether. We receive in this case the particularly long streams, for which the coronas of some eclipses are famous.

(ε) If the surface from which the stress proceeds be discontinued in some places; if, for example, one fastens a small piece of tin foil on the metal ball, then a stream goes out from its point, but on both sides of it are formed deep black rifts, which extend down to the surface of the ball. These are the well known rifts of the corona.

(ζ) When another conducting mass is brought near the ball, for instance a metal ball suspended by a silk thread, then the streams go out vertically from the surface of the first ball, and bend themselves towards the second, which they endeavor to meet vertically also. In this way the bent rays of the corona arise converging to one point without the Sun. Great masses of iron meteors in the Sun's vicinity must cause such a bending of the corona's rays.

(η) In atmospheric air the streams have a pale red color; in rarefied hydrogen they show a pale silver color; the spectrum is continuous in this case and the slit must be made pretty wide in order to see anything at all. This fact also seems to me very important for the elucidation of the corona, whose spectrum is well known to be continuous, because it is probably only rarefied hydrogen, which fills up the upper regions at great distances from the Sun's apparent surface.

At lower pressure the aureole divides into different parts; with air a thin bluish layer of light lies next to the ball, then comes a dark dividing space, and then the red rays begin. Of these phenomena, called "cathode phenomena," with the ordinary experimental arrangements we cannot see anything near the Sun. But we must not forget that we do not see at all the real origin of the corona rays. For the Sun's surface is probably only an optical phenomenon, as has already been mentioned. But the "cathode phenomena," never appear in free gas

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space, but only on the boundary of a luminous gas and a rigid body, conducting or non-conducting, as we have shown in the above mentioned treatises.

I should like to take this opportunity of observing that one also obtains with the same apparatus, phenomena which agree altogether with the appearance of comets; standing apparently in the most intimate connection with the Sun's activity. If one attaches on an insulating support, such as a glass rod, a non-conducting body (wax ball) near the metallic ball, then rays arise bending themselves around the wax ball towards the glass behind it, especially when there is an outer shell connected with the earth. One sees a phenomenon like a comet's tail accompanying the wax ball, turned away from the metallic ball in the centre, from which the electric excitement emanates. It is also probable that in comets non-luminous matter comes out of the head and becomes luminous only by rapidly changing electric polarizations caused by electric disturbances in the Sun, like the rarefied gases in our electrodeless vessels.

From this electro-magnetic point of view also, the connection of the Sun's activity and the Earth's magnetism is plausible. This connection was recently denied altogether by some authorities, for example by Lord Kelvin; but the manifold indications cannot be overlooked which are afforded by careful observations, old as well as new, such as the very valuable daily records of the Sun's surface obtained at Kenwood Observatory by Professor George E. Hale, and compared with the magnetic records of the U. S. Naval Observatory, or with the curves obtained by Father Sidgreaves at the Stonyhurst Observatory.

It must be admitted that no magnetic distribution, however created, when rotating on the face of the Sun, can explain the variation of the elements of terrestrial magnetism. We must therefore seek other explanations, and in this respect the above detailed electro-magnetic theory recommends itself. If the Sun is really the seat of electro-magnetic disturbances, then it must necessarily be the source of electro-magnetic radiation. The Earth is a conductor; viewed as an electric oscillator its own period is $\frac{1}{17}$ of a second. The oscillations proceeding from the Sun are composed, like all other oscillations, of an electric vector, varying periodically with time and place, and of a similar magnetic one. Therefore the electro-magnetic streams of the Sun must act upon the magnetic condition of the Earth. In this connection we must take into account the fact that the magnetic vectors of the Sun's streams make very different angles with the

magnetic vector of the Earth at different times, and that therefore the processes going on in the Sun are affected very differently by the various elements of terrestrial magnetism.

Since it has been demonstrated that electro-magnetic radiation is very closely related to the radiation of light and heat, there can no longer be any doubt that this also will play a great part in astro-physics.

STARS HAVING PECULIAR SPECTRA.*

M. FLEMING.

An examination of photographs of stellar spectra recently received from Arequipa, Peru, taken under the direction of Professor S. I. Bailey, and forming part of the Henry Draper Memorial, has added the objects of interest given in the following table to the list of those already known. The designation of the star is given in the first column, and is followed by its approximate position in right ascension and declination for 1900. The catalogue magnitude is given in the fourth column and a brief description of the spectrum in the last column.

Design.	R. A. 1900		Dec. 1900		Mag.	Descr.
	h	m	°	′		
A.G.C. 6519	5	31.7	−25	48	8	Type IV.
D.M. −29° 2538	5	47.2	−29	19	9.3	Type III, hydrogen lines bright.
A.G.C. 8670	6	51.3	−42	14	6.7	Type IV.
B.D. −10° 1786	6	53.2	−10	38	7.3	F line bright.
A.G.C. 13554	9	51.6	−57	15	8½	Type V.
A.G.C. 13665	9	56.7	−59	44	7¾	Type IV.
— —	10	6.1	−62	5	—	Type V.
— —	10	14.4	−62	9	—	Type V.
— —	10	22.9	−58	8	—	Type V.
A.G.C. 19745	14	29.5	−42	56	8½	Type IV.

D.M. − 29° 2538 has a photographic spectrum of the third type having also the hydrogen lines bright. Measures of the brightness of this star on photographs taken October 15, October 21, November 3, 1889; December 8, 1892; and March 18, 1893, give the magnitudes 9.4, 9.6, 10.0, < 11.4, and 7.9 respectively, thus proving its variability.

A.G.C. 13554, and the stars whose approximate positions for 1900 are in R. A. $10^h\ 6^m.1$, Dec. − 62° 5′, R.A. $10^h\ 14^m.4$, Dec. − 62° 9′, and R. A. $10^h\ 22^m.9$, Dec. − 58° 8′, have spectra of the

* Communicated by Edward C. Pickering, Director of the Harvard College Observatory.

fifth type, consisting mainly of bright lines, and these added to those already announced increase the known number of these objects to fifty-five. The galactic longitudes of these stars are 248° 43′, 253° 0′, 253° 51′, and 252° 42′, and their galactic latitudes − 2° 19′, − 5° 14′, − 4° 46′ and − 0° 46′ respectively.

HARVARD COLLEGE OBSERVATORY,
Cambridge, Mass., September 12, 1893.

THE SPECTRA AND PROPER MOTIONS OF STARS.*

W. H. S. MONCK.

I had hoped to be able to contribute a further paper dealing pretty fully with the spectra and proper motions of stars but want of time has hitherto prevented me. Some months ago, it occurred to me that there was possibly a mistake in the *Draper Catalogue* as to the spectra of certain stars with considerable proper motion which were described as Sirian; and on communicating my doubts, Professor Pickering had the spectra re-examined and in the kindest manner sent me a complete list of his results with liberty to publish them. The result was that the proportion of these stars which passed over to the solar class was even larger than I had expected. The readers of ASTRONOMY AND ASTRO-PHYSICS will very probably wish to see the details and to correct their copies of the *Draper Catalogue* accordingly. They are as follows (when two kinds of spectra are mentioned, the meaning is that the true spectrum is intermediate between them):

Number in Draper Catalogue.	True Spectrum.	Number in Draper Catalogue.	True Spectrum.	Number in Draper Catalogue.	True Spectrum.
124	G–H	3573	F	6828	F
236	F–G	3645	F	6864	F
511	F	3840	F	(7188	A–F)
607	F	3850	F	7539	F
707	F	4207	F	8288	G
892	F	4229	F–G	8578	K
899	F	4495	F	8656	I
1195	F–G	5081	F	(8726	A–F)
1267	G	5243	G	8792	K?
1287	F–G	5379	F–G	8865	F
(1583	A–F)	5469	F	9028	F
1726	F	5624	E–G	9163	G–H
2078	F–G	5692	F	9166	G–H
2476	F	5762	F	9200	F–G
2692	G	5924	G	(9842	A–F)
2824	F	6156	F–G	9938	F
(3435	A–F)	6159	F	10071	F
3446	F–G	(6176	A–F)	10134	H
3569	F–G	6802	F		

* Communicated by the author.

More than one-half of the stars about whose spectra I inquired on account of their large proper motion appear in the foregoing list. Most of them were so faint that astronomers had already been cautioned against relying on the spectra given in the *Draper Catalogue*. Still I hardly expected that 60 per cent of the stars in my list would pass from one class to the other. Among those which thus pass over is the solitary star with spectrum B whose proper motion appeared to be considerable. Its true spectrum is F.

I re-examined the stars in the *Cincinnati Catalogue* whose spectra I had been able to identify in the *Draper Catalogue*—making these corrections. The revised numbers were the following: Capellan 275, Arcturean 157, Sirian 42. There were 5 stars of the Antarian type, 5 intermediate between Sirian and Capellan and 3 between Capellan and Arcturean. None of the Sirians, as already noticed, are of the type B.

I also submitted a list of Capellan stars which from their magnitudes and small proper motions I suspected to be really Sirians. In this instance, however, the spectra given in the *Draper Catalogue* were in the great majority of instances confirmed. The most noteworthy exception is β Orionis (or Rigel) whose spectrum is not F but B. Others which pass to the Sirian class are p Tauri, 42 Aquilæ, 1 Camelopardi, μ Sagittarii, 59 Piscis, 55 Cygni, f_B^1 Cygni, 18 Aquarii, 4 Pegasi, 18 Pegasi (B), 26 Cephei and c Piscis. It seems clear that there are several fairly bright Capellan stars whose proper motion is very small. This may arise from their motion being nearly in the same direction with that of the Sun, in which case they may have sensible parallaxes notwithstanding their small proper motion. At present we can only conjecture; but I recommend all fairly bright stars with the spectrum F to the attention of parallax-hunters.

ON THE APPLICATION OF DOEPPLER'S PRINCIPLE TO THE MOTION OF BINARY STARS AS A MEANS OF IMPROVING STELLAR PARALLAXES AND ORBITS, AND AS AN ULTIMATE MEANS OF TESTING THE UNIVERSALITY OF THE LAW OF GRAVITATION.*

T. J. J. SEE.

It is well known that the orbital revolution of binary stars does not enable us to decide as to the law of attraction govern-

* Read before the Section of Astronomy, Chicago Academy of Sciences, Oct. 3, 1893.

ing their motion. In the planetary system the demonstration of the law of gravitation rests upon the laws of Kepler, which are found by observation to be very exactly fulfilled by the motion of the planets round the Sun. If the law of Newton did not hold with the greatest exactness, the effects would soon become very sensible in the motion of the perihelia of the planets;* and since (with the exception of Mercury, the anomalous motion of whose perihelion can be otherwise explained) actual observation does not disclose any such irregularities in the planetary motions, we may confidently believe that in our system the law of Newton is rigorously the law of nature.

In the stellar systems, however, the case is different, as observation merely shows:—

(1). That the apparent orbit of the companion is an ellipse;

(2). That the apparent radius vector describes equal areas in equal times.

It is found by observation that the principal star is not generally in the focus of the apparent orbit, and hence we cannot infer the existence of the law of gravitation.

But since the areas described by the radius vector are proportional to the time, we may infer that the motion is in a plane, as is indeed observed to be sensibly the case in the system of 42 Comæ Berenices, whose orbital plane passes approximately through the Sun. From this it follows that the force is central, but as many other forces besides Newtonian attraction can cause the companion to describe an ellipse, we can demonstrate the existence of the law of gravatation only by showing that the central star is in the focus† of the real ellipse, which requires us to know the inclination of the orbit.

In determining double star orbits it is customary to assume the operation of the law of gravitation, and the data of observation are then sufficient to enable us to find the elements of the real orbit. But as we thus employ the law of gravitation in deriving the inclination and other elements of the orbit, we cannot regard the inclination as independently known. Hence we must resort to the spectroscopic application of Döppler's principle to the relative motion of the two stars in the line of sight, as a means of testing the inclination derived from the theory of gravity. The relative displacement of the spectral lines of the two

* *Mecanique Celeste*, Liv. II, ch. I, § 6.

† See the article of Professor A. Hall in Gould's *Astronomical Journal*, Vol. VIII, No. 177; also Tisserand's *Mecanique Celeste*, tome I, ch. I; or the writer's small monograph "On the Law of Attraction in the Stellar Systems" (Berlin, 1890).

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stars will give the relative motion of the companion in the line of sight, freed from the effects of the proper motion of the system in space as well as from the effects of the motion of the Earth and Sun.

Thus, suppose the companion to be moving in its orbit near the ascending or descending node. It will then have a relative motion towards or from the Earth equal to

$$\varkappa = v \sin i,$$

where v is the velocity in the orbit, and i the inclination of the plane of the orbit to the tangent plane of the heavens. For any other point than the node the component of the motion in the line of sight would be

$$\varkappa = v \sin i \cos u$$

where u is the argument of the latitude.

Now if we take a system which is highly inclined like α Centauri, we see that $\sin i$ is very large and hence nearly the whole motion at the nodes will be towards or from the Earth. A simple calculation shows that the velocity of the companion of α Centauri varies from 5.6 to 18 kilometres (*i. e.* 3.5 to 11 miles) per second. At the ascending node the velocity in the line of sight will be at least 15 kilometres per second, and as the stars are about 9″ apart, this motion could easily be detected by modern spectroscopic apparatus. In case of α Centauri, the relative motion of the companion in the line of sight will never be much less than 5 kilometres per second, and hence if this star were accessible it would be particularly well suited for testing the law of gravitation. The great difficulty with most stars will lie in the unknown or inaccurate parallax and in the closeness and inequality of the components.

If we take the case of 70 Ophiuchi, we find by means of Krueger's parallax (0″.162) and Burnham's orbit, that the average orbital velocity of the companion is about 9.5 kilometres per second. The average relative velocity of the companion in the line of sight will be about 8.5 kilometres, which ought to be detected by modern apparatus. The practical difficulty of detecting this motion would be increased by the inequality of the components, but this obstacle may not prove insurmountable.

Now to test the law of gravitation it is only necessary to compare the spectroscopic velocity in the line of sight with the velocity computed from the theory of gravitation by means of the ascertained parallax of the system. If the results agree, we may conclude that the law of gravitation is as accurate as the other elements entering into our calculation. If the results disagree to

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a marked extent, we may look for some error in the parallax of the system (this is the weak point), or in the elements of the orbit, or lastly in the law of gravitation.

Thus spectroscopic observations will serve to check the parallaxes of binary systems and to improve their elements, as well as to confirm the law of gravitation. It is to be hoped that some great Observatory will take up the spectroscopic investigation of the motions of binaries and their parallaxes. These lines of research may confidently be expected to greatly increase our knowledge of other systems in space.

THE UNIVERSITY OF CHICAGO,
1893, Aug. 31st.

AN ABSOLUTE SCALE OF INTENSITY FOR THE LINES OF THE SOLAR SPECTRUM AND FOR QUANTITATIVE SPECTRUM ANALYSIS.*

L. E. JEWELL

A "long felt want" in spectroscopic work has been a scale of intensity, which should be reasonably accurate, and constructed upon some rational sort of a basis.

During the last few years while engaged upon the determination of the wave-length and character of all the lines in the solar spectrum, (a work carried on at the Johns Hopkins University under the supervision of Professor H. A. Rowland), I have often felt the need of some definite sort of a scale for the estimation of the intensity of the solar lines, while the inaccuracy and the arbitrary and unsatisfactory character of eye estimates has continually become more evident.

It was while in this state of mind that in January, 1892, Professor Harrington, chief of the U. S. Weather Bureau, wrote to Professor Rowland to see if an investigation of the "*rain band*" could not be undertaken, upon a large scale and by photographic means, at the physical laboratory of the Johns Hopkins University.

I made the investigation, and the scale of intensity which forms the subject of this paper was one of the results.

In making observations upon the "rain band," comparisons of intensity were made between solar and water-vapor lines, of nearly equal intensity, and as near to each other as it was possible to find satisfactory lines.

* Read at the Congress of ASTRONOMY AND ASTRO-PHYSICS, Chicago, August, 1893.

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This was the only method at all practicable, but to get out of this any result which should be other than mere guess-work, it becomes necessary to in some way calibrate both the solar and water-vapor lines which were used in the comparisons. The plan adopted was to make a photographic scale, consisting of a series of lines formed by photographing the image of a narrow slit, the exposures being related to each other in geometrical ratio, the ratio adopted being the square root of two, so that every other line would be related in the ratio of 1, 2, 4, 8, etc.

A scale thus constructed as a trial was found to have its lines not sufficiently like those of the solar spectrum to make comparisons of any value, and the plan adopted to correct this defect was to place in front of the photographic lens a wire screen sufficiently fine to produce slightly diffuse lines, but not so fine as to produce troublesome diffraction bands.

In this way a scale was produced in which the lines had nearly the appearance of the better class of solar lines, and which varied in intensity according to some rational and definite rule.

The first complete scale thus produced has a number of defects which would be expected in any first production, but it has answered the purpose so well, that, taking into consideration the difficulties in producing a more perfect scale, and not having some of the apparatus necessary to the production of a more satisfactory one, and being pressed for time, I made the calibration of the comparison lines used in the investigation with the original scale.

The principal defects referred to are, first, an uneveness in the width and consequently in the intensity of the different portions of the same line, due to defects in the slit used; and secondly one due to the turning of the camera upon its axis horizontally, in order to place the image of the slit in its proper place upon the photographic plate, thus introducing an error caused by the field of the photographic lens being unequally illuminated. This can be avoided by moving the photographic plate across the field. A satisfactory scale can be constructed by using a slit, made by cutting with a thin, sharp, knife edge, a straight narrow line through the film of a blackened photographic plate; or by photographing a wire stretched against the sky or any other bright even background, and using the resulting negative as a slit. The intensity ratio of the scale can be made anything that is desired by prop-

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erly adjusting the development and exposure ratio.* We thus have a scale which resembles the better class of solar lines pretty closely and which can be relied upon to have the intensity ratio of the scale a *constant*, for this is the important feature of a scale and not the numerical value of the ratio. This is not strictly true but for the object for which the scale is to be used, and considering the range of intensity likely to be used, it is closely approximate. Considering the deposite of silver in the lines of the photographic scale to have been directly as the length of exposure, the ratio of intensity will be according to the law of absorption, which is slightly different from a geometrical series. As this matter is carefully gone into in a paper to be published by the U. S. Weather Bureau I need say nothing farther than that the law governing the intensity of the lines in the scale and the law of absorption for lines such as those in the atmospheric spectrum are the same, the numerical value of the ratio only being different. Measurements made by the scale can be reduced almost as readily as though the intensities of both kinds of lines varied according to a geometrical ratio. The numerical value of the intensity ratio or the gauge of the scale can be determined by the bolometer or by other means, but a much more important thing is to determine its value in terms of the absorption produced by different quantities of the substance, the absorption lines of which it is desired to investigate.

In an investigation of the atmospheric lines, a quantitive calibration of the scale can be made, by determining the amount of material through which the sunlight has passed at the time the observations for calibrating the scale are made.

The oxygen constituent of the air offers a ready means of doing this, as it varies little from day to day, and the amount of oxygen (for preliminary work the whole atmosphere may be considered as of constant constitution) traversed by the sunlight at different elevations of the Sun has been calculated by Forbes and Bouguer.

In observing, the scale is placed in the focus of the spectroscope, and the line which it is desired to measure is brought between the two lines of the scale which are nearest to it in intensity.

* A scale constructed in this way, but using a small round hole instead of a slit in front of the source of light, and with the screen in front of the photographic lens as before, might form a very useful scale for the determination of the photographic magnitudes of stars, or rather the inverse of such a scale. By the proper adjustment of the screen the appearance of the photographic image of a star could be imitated very closely indeed, and the intensity ratio of the scale could be made anything desired.

A scale constructed upon this principle could also be made very useful in the visual determination of magnitudes.

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It might be supposed that measuring under different conditions with different gratings and in different orders of spectra, might cause endless confusion and difficulties, but this is not the case.

The scale intensity of a line depends upon the *quality* of the spectrum produced by a grating, as regards definition and amount of diffuse light, also upon the dispersion used and upon the width of the slit, and though the measurements made upon a series of lines under different conditions will not give the same values in each case yet the measurements are strictly comparable as the differences are constants which depend upon the width of slit, quality of spectrum, and dispersion used, so that they can be reduced to whatever conditions are taken as standard.

In this way the laws governing the absorption of light by gases can be studied and the intensity of the lines of the solar spectrum can be determined upon a scale which really means something, which can be used for quantitative spectrum analysis, and which may justly be considered an absolute scale, as any errors to which it may be subject can be determined, and allowed for. It can be used for both eye and photographic determinations. In the latter the scale can be placed in contact with the photographic plate and an exposure made just as in photographing the solar spectrum, the result being the lines of the scale along with the solar lines. A better way for most purposes, however, would be to photograph the solar spectrum on one part of the plate, and then with the exposed portion protected, photograph the lines of the scale upon the unexposed portion of the plate, using the light of the solar spectrum but interposing a strip of ground glass to diffuse the light and make the background more uniform.

Additional exposure should be given when photographing the lines of the scale in order to make the background of the same intensity in both strips, and this ratio should be followed in all determinations with the same apparatus in order to have the comparisons comparable, except that where there are many lines in the part of the spectrum to be measured, sufficient additional exposure should be made to make up for the loss of light caused by the lines, and to make the background of the two strips of equal intensity. This only applies where the ground glass is used.

A negative of the scale can be used for photographic determinations of the intensity of the metallic lines.

To show the results of quantitive measurements, I give, by permission of Professor Harrington some few results obtained in the investigation of the "rain band."*

* To be published by the U. S. Weather Bureau.

Curve 3 represents the amount of atmosphere passed through by sunlight at different altitudes of the Sun, where unity equals the amount passed through with the Sun overhead. Curve 1 is the intensity curve of the oxygen lines as measured upon the scale where unity represents the intensity of the lines with the Sun overhead. In like manner curve 2 represents the intensity curve of water-vapor lines for winter and curve 4 for summer, where in each case unity represents what would be the intensity with the Sun overhead. It is evident that curves 1 and 3 are related in some way, and a study of the curves would show that the relation is the same as that of two geometrical series with different ratios. Curve 1 is what would be produced were the ratio of absorption the same as that of the material producing absorption, and if the intensity ratio of the scale used were about 1.8 instead of 1.414.

The curve is also what would be produced were the intensity ratio of the scale 1.414 and the absorption ratio 1.51 instead of 2 for double the amount of absorbing material. The intensity ratio of the scale may be slightly larger than 1.414 but is certainly not as large as 1.8, consequently if sunlight passes through two masses of absorbent gas (oxygen in this case) one of which is double the other, the ratio of the absorption will be less than two, and this is what would have been expected theoretically.

The scale has never been calibrated for the intensity ratio of the series of lines composing it, consequently I can not give the absorption ratio for oxygen, but for the present purposes this is not necessary as we have already made a *quantitive* calibration of the scale, and if we raise all the results obtained with the scale to the 1.8th power, we will obtain results which correspond to quantities of material producing the absorption.

Thus as an instance with the Sun overhead, the first line in the tail of the α group will read 4 on the scale used, and this corresponds to a beam of sunlight of 1 centimetre square section passing through 1.033 kilogrammes of air or 0.2356 kilogrammes of oxygen. When the Sun is on the horizon, the line will read 18.6 on the scale used. This means that the line is 7.8 times as intense in scale value, and raising this to the 1.8th power we obtain 38.57, or in other words, the sunlight has passed through 38 times as much material as when overhead, or a beam of one square centimetre section has passed through 8.88 kilogrammes of oxygen.

If we examine curves 2 and 4, we see that the form of curve is

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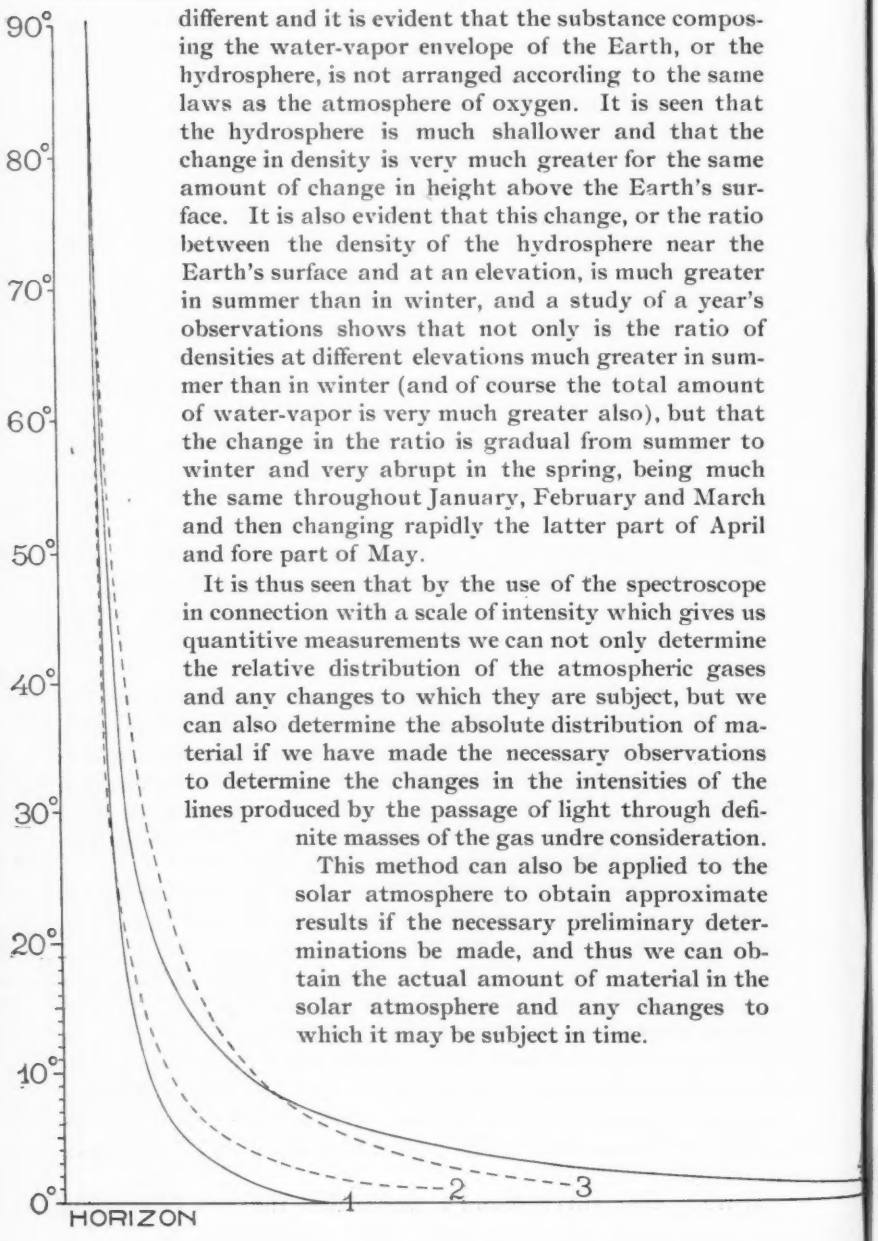


different and it is evident that the substance composing the water-vapor envelope of the Earth, or the hydrosphere, is not arranged according to the same laws as the atmosphere of oxygen. It is seen that the hydrosphere is much shallower and that the change in density is very much greater for the same amount of change in height above the Earth's surface. It is also evident that this change, or the ratio between the density of the hydrosphere near the Earth's surface and at an elevation, is much greater in summer than in winter, and a study of a year's observations shows that not only is the ratio of densities at different elevations much greater in summer than in winter (and of course the total amount of water-vapor is very much greater also), but that the change in the ratio is gradual from summer to winter and very abrupt in the spring, being much the same throughout January, February and March and then changing rapidly the latter part of April and fore part of May.

It is thus seen that by the use of the spectroscope in connection with a scale of intensity which gives us quantitive measurements we can not only determine the relative distribution of the atmospheric gases and any changes to which they are subject, but we can also determine the absolute distribution of material if we have made the necessary observations to determine the changes in the intensities of the lines produced by the passage of light through definite masses of the gas undre consideration.

This method can also be applied to the solar atmosphere to obtain approximate results if the necessary preliminary determinations be made, and thus we can obtain the actual amount of material in the solar atmosphere and any changes to which it may be subject in time.

In constructing an intensity scale for the solar spectrum, it would probably be well to take as unity the first line in the tail of the α group when the Sun is overhead, or in other words the value of the line in question for one atmosphere.

Of course in measuring the intensity of a line neither the width nor relative darkness of the line is to be considered alone, but the whole effect or the integral of absorption.

In a determination of intensity it is best to avoid as far as possible eye estimations of differences of intensity; but where two lines are the same or very nearly the same intensity the eye is pretty reliable, consequently a scale should have the ratio between the intensities of adjacent lines as small as the eye can readily perceive.

The lines of the solar spectrum or of the different bands of oxygen furnish sufficient material for either the testing or calibration of a scale if their intensities were once satisfactorily determined.

MARIETTA, Ohio, Aug. 18th, 1893.

HELIOGRAPHIC LONGTIUDES REFERRED TO THE SOLAR MAGNETIC PRIME MERIDIAN.*

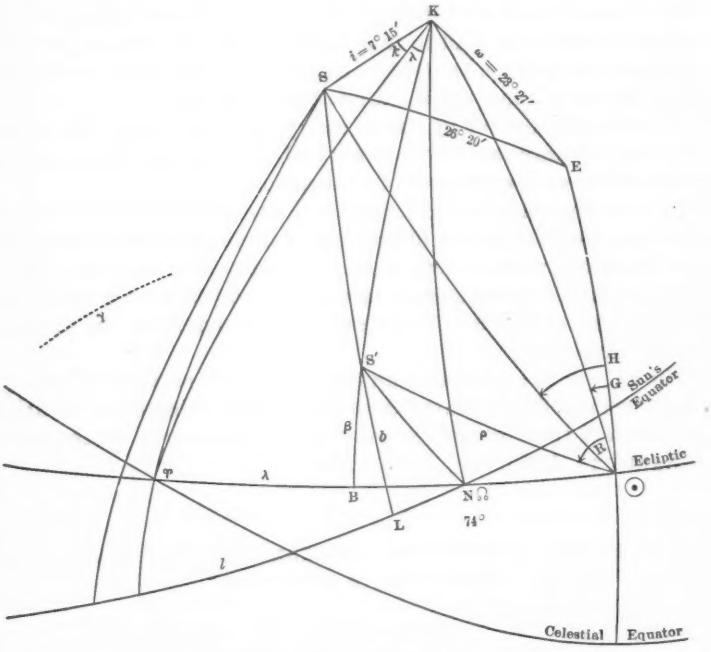
FRANK H. BIGELOW.

Heliographic longitudes for Sun spots and other solar phenomena have been referred to the prime meridian established by Carrington as the result of his Red Hill observations. It assumes that the period of rotation of the sun-spots at a certain latitude is the same as that of the body of the Sun, and that the solar equator revolves more rapidly, the reason for this being unknown. There has been no way to test these assumptions, and consequently the system of longitudes proposed by Carrington has been in use for forty years, adopted into astronomy as he gave it. The recent application of terrestrial magnetic variations to the state of the permanent magnetism of the Sun,† gives us, however, an opportunity to view the problem in a new light, and questions the soundness of Carrington's position.

In order to state clearly the two sides to the argument, the usual method of computing heliographic longitudes will be briefly given, in a notation which agrees with the symbols of my preceding papers.

* Communicated by the author, by permission of the Chief of the Weather Bureau.

† See ASTRO-PHYSICS, Oct., 1893, and the *American Meteorological Journal*, Sept., 1893.



K = Pole of Ecliptic, S = Pole of Sun's Equator, E = Pole of Earth's Equator, Vernal Equinox at sign Aries, N = Longitude of Ascending Node of Sun's Equator on the Ecliptic = 74°, ⊙ = Center of Sun's disk as seen from the Earth = Heliocentric Longitude 180 + ⊙. G = Auxiliary Angle E⊙K, H = Auxiliary Angle E⊙S. From sign Aries to B = λ Heliocentric Longitude, and BS′ = β Heliocentric latitude of spot S′; L = NL, Heliographic longitude of the Spot S′.

Given the Position Angle, R = K⊙S′, and distance from centre of disk ρ (corrected each), *to compute l, b.* K⊙ = 90°. B⊙S′ = 90° − K⊙S′ (direction of position angle NE SW), and ⊙S′ = ρ in measured linear distance.

$$\sin \odot S' = \frac{\odot S'}{\text{Sun's semi-diameter}}$$

sin BS′ = sin ⊙S′ sin B⊙S′.
tan B⊙ = cos B⊙S′ tan ⊙S′.
BN = N⊙ − B⊙
N⊙ = 180 + ⊙ − N (where ⊙ = longitude of the Sun at date).
cos NS′ = cos S′B cos BN.
cos S′NB = cot NS′ tan BN.
S′NL = S′NB + 7° 15′
sin S′L = *sin b* = sin NS′ sin S′NL.
sin NL = *sin l* = tan S′L cot S′NL.

Carrington took for his prime meridian that which passed through the vernal equinox for 1854.0 that is from S to sign Aries; for the period of rotation, sidereal 25.38 days or synodic 27.27526 days, equivalent in mean daily motion to 14°.1844 = 851′.064. This prime meridian comes to the central meridian on such dates that Nov. 9, 1853, may be adopted as the beginning of revolution No. 1, and the direction of reckoning longitudes being in that of the rotation of the Sun on its axis, this meridian will be in longitude 14°.1844 at the beginning of the 2° day, 28°.3688 at that of the third day, etc., apparently to the west of the centre of the disk. Therefore the longitude of the prime meridian from node,

$$l' = T\,\frac{360^\circ}{25.38},$$

where T is the number of days elapsed since the prime meridian is central. Finally the heliographic longitude of the spot from the prime meridian $= l - l'$.

The following table gives the result of the observations of Carrington and Spoerer for mean daily motion of the sun-spots in longitude at different latitudes.

Latitude.	Carrington's daily motion.	Spoerer's daily motion.
°	′	′
0	867	881.5
2	866	874.3
5	864	861.6
7	861	858.6
9	858	853.6
12	854	851.9
14	850	843.5
16	847	837.5
18	842	832.9
21	837	832.1
25	830	827.0
30	818	823.9

The mean daily motion 14°.1844 (Carrington) therefore corresponds to latitude 13°.5, and 14°,2665 (Spoerer) to latitude 10°.5 and they represent the *average rate of motion of the whole body of Sun-spots.*

Carrington's motion at the equator is 867′.					
Spoerer's	"	"	"	"	" 881′.5.
Faye's	"	"	"	"	" 863′.
Tisserand's	"	"	"	"	" 858′.

No arguments have ever been presented to show that the adopted rotation corresponding to latitude 10° — 13° is that of the body of the Sun, rather than the other rates deduced for the equator itself. On the face of the problem all the theoretical arguments are in favor of the equatorial rate, since, as is so well

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known in meteorology from the work of Ferrel, Helmholtz, Oberbeck, and many others, a rotating spherical body having fluid or gaseous constituents will set up surface currents, those at the equator having nearly the same rate of angular motion as the solid nucleus, those at 10° — 15° north or south latitude having a steady anti-rotational drift relatively to the equator. Hence it is natural to suppose that the sunspots do rotate more slowly than the equator, being transported in this anti-rotational current, of which we see an example in the tropical trade winds of the Earth.

I have already announced my result, the details of which will be published soon, for the angular motion of the permanent magnetic system of the Sun, which is supposed to reside in a rigid nucleus. Since the magnetic output leaves the Sun near the the magnetic poles and therefore within a few degrees of the poles of rotation, the two axes being separated by about 4½°, we must ascribe the resultant motion to the polar regions of the Sun. This angular velocity is 868′.758, and it will be observed that it is nearly the same as that adopted by Carrington, 867′, the mean value of the authorities quoted being 867′.4. The result of the magnetic work depends upon the total mass of observations of the magnetic and meteorological observatories of Europe and the United States, covering the period 1878 to 1892 inclusive, and has the same scientific value as the observations of sunspots, since the computations can be verified at any time. This close agreement between the polar and the equatorial daily motions is a strong argument in favor of the view that the sunspots flow anti-rotationally past the equator, the true period of solar rotation being represented by the values 867′ or 868′.

The epoch chosen for the magnetic ephemeris is 1887, June 12.22, and corresponds to the instant, Greenwich mean time, when the solar meridian passing through the south coronal or magnetic pole is central on the disk. There has long been a need in solar physics that a permanent physical meridian be localized on the Sun to which solar phenomena can be referred. This adopted meridian is analogous to a plane on the Earth through the geographical pole and the magnetic pole of North America (negative, the south coronal pole of the Sun being also negative), and using it for the plane of reference, instead of Greenwich. The motion of magnetic poles, if any exists, would be an objection to the permanent verification of such a meridian, but the one passing through such a pole at an epoch, would be the proper basis for investigating the problem of the secular variation.

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It will be found that Carrington's prime meridian and the proposed magnetic meridian coincided with each other on June 19.16, 1883, October 12.76, 1886, and February 5.15, 1889, G. M. T., the interval being 1211.35 days, from which other dates o-coincidence can be computed.

It would seem more natural also in the classification of solar phenomena, to adopt an antirotational system of longitudes, as on the Earth, the west longitudes being positive. Therefore the longitudes for the beginning of each day, Greenwich mean noon, counted on the surface of the Sun in a direction opposite to Carrington's, will be as in the following table.

LONGITUDES ON THE SUN REFERRED TO THE MAGNETIC PRIME MERIDIAN THROUGH THE SOUTH CORONAL POLE.

SYNODIC DAILY MOTION 13°.4936.

Days.	Degrees.	Days.	Degrees.	Days.	Degrees.
0	0°.0000	10	134°.9360	20	269°.8720
1	13.4936	11	148.4296	21	283.3656
2	26.9872	12	161.9232	22	296.8592
3	40.4808	13	175.4168	23	310.3528
4	53.9744	14	188.9104	24	323.8464
5	67.4680	15	202.4040	25	337.3400
6	80.9616	16	215.8976	26	350.8336
7	94.4552	17	229.3912	27	364 3272
8	107.9488	18	242.8848		
9	121.4424	19	256.3784		

The tabulation of solar phenomena in this way is perfectly simple, it being necessary to reduce the observed position at a given instant of time to the central meridian of the date, and then tabulate the same by the longitudes of the synodic rotation.

The dates for the beginning of the coronal magnetic periods are derived from the epoch 1887, June 12.22, with period 26.68 days.

DATE OF BEGINNING OF FIRST PERIOD IN EACH YEAR.

1880	Jan.	25.54	1890	Jan.	1.02
1881	Jan.	6.38	1891	Jan.	9.54
1882	Jan.	14.90	1892	Jan.	18.06
1883	Jan.	23.42	1893	Jan.	25.56
1884	Jan.	5.26	1894	Jan.	7.32
1885	Jan.	12.78	1895	Jan.	15 84
1886	Jan.	21.30	1896	Jan.	24.36
1887	Jan.	3.14	1897	Jan.	5.20
1888	Jan.	11.66	1898	Jan.	13.72
1889	Jan.	19.18	1899	Jan.	22.24

I found it impossible to obtain any periodic result from Sunspots, auroras, magnetic storms, and meteorological variations, when using the sunspot period of Carrington, but on the other

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hand all these phenomena reduced to a consistent inter-related system when the equatorial rate was laid at the basis of the investigation. It will be most interesting to know whether the Sun does not dispose the spots, the faculæ, the prominences, in such habitats as correspond to the magnetic meridians, whose relative strength has already been indicated in the publications referred to above.

THE PHYSICAL CONSTITUTION OF THE SUN.*

WALTER SIDGREAVES.

The year 1859 has become memorable in the history of astronomy, as the epoch of a new departure in the science of the heavens. Newton had dimly forecasted it from his great discovery of the composite nature of ordinary light; Wollaston had divined the secret of the right analysis of the Sun beam, when in 1802 he substituted the straight slit for Newton's round hole in the shutter; and Fraunhofer had introduced the solar hieroglyphics to the speculation of astronomers in 1814, when he gave them his map of some 600 dark lines crossing the solar spectrum. But it was not until 1859, after 45 years of waiting amidst the puzzled conjectures of physicists, that the meaning of the lines was clearely deciphered, and the instrument which showed them was proved, by the classical experiments of Kirchhoff and Bunsen, to be the far-reaching analyzer of the chemical laboratory.

The first and fundamental lesson of the new teaching was what may be termed the specific radiation and selective absorption of matter in the gaseous state, as opposed to its universal radiation in the liquid or solid state at incandescence. The unquestioning acceptance of this principle, at its first enunciation by the eminent professors of Heidelberg, stamped it with the mark of truth. Its simplicity, and its perfect harmony with the growing insight into the nature of heat and light left no room for hesitation. In the more attenuated and delicate form of existence, which exhibits the properties of a gas, the molecular vibration is incapable of exciting, in the universal ether, any but its own periodic vibrations; while in the grosser form of the liquid or the solid state, its vibrations, according to their energy, excite indiscriminately every degree of oscillation frequency.

The immediate deduction from this principle was the broad

* Communicated by the author.

statement of the physical constitution of the Sun; that it must be a solid ball, or a liquid sea at white-heat incandescence, surrounded by a heated gaseous atmosphere containing the vapors of many metallic substances. This statement of the Sun's condition held its place amongst the accepted certainties of celestial physics until the year 1865, when it received a modification which in no way contravened the principle of Kirchhoff and Bunsen. It was well known that the bright illumination of the candle flame or of the coal gas flame was due to the incandescence of a cloud of small carbon particles floating in the heated, gaseous envelope of the flame; and this form of the solar photosphere came into favor in the year 1865. Father Secchi seems to have been the first to suggest it in January, 1864;* but the novel conception of its origin as expounded to the Academy of Sciences in 1865, by M. Faye, together with the opposite account of its formation supported by the Kew observers, brought the subject before the judgment of solar physicists. The two theories of the cloud formation appeared almost simultaneously. The Kew observers had their proposition ready for the press at the time of M. Faye's publication in the *Comptes rendus*. But the two authors of the modification of Kirchhoff's photosphere had moved on different lines. M. Faye, following up a consideration of the effects of the intense heat of the interior solar mass, concluded that matter, there, must be in an ultra-chemical condition, in a primordial state or state of dissociation, incapable of energetic or universal radiation; and that there must be a limiting sphere where association and condensation would be continuously exchanging with vaporization and dissociation. The Kew observers on the other hand considered only the atmospheric state of the solar orb; and, reasoning possibly from our own climatic action, concluded also that there must be continuous exchanges between condensation and vaporization. Thus both schools arrived at the same cloud formation of the photosphere from opposite startings; the French savant rising to it from the interior condition of the Sun, the English descending to it from that of the outside surroundings.

The formation of spots upon the surface of the Sun naturally became the test of the rival methods. Each school following its own line of thought derived the origin of the spot from its own starting point. Faye's conception of these was a rent in the luminous cloud by an outburst of heated gases clearing away the incandescent condensations and exposing to our view the interior

* *Bull. Meteorologico*, Jan. 1, 1864, p. 4.

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feebly radiating dissociated matter; while Dr. Balfour Stewart, with his colleagues de la Rue and Lowey, attributed the dark areas to the cooler gases descending upon the photosphere. Both authors accepted ascending and descending currents in connection with spot-formation; but from the beginning the issue between them was clearly lined between the two currents, or as Mr. Lockyer has it, between a heat too great and a heat too little for a radiative energy equal to that of the photosphere.

Faye's hypothesis was in itself a tempting one. The intense heat of the Sun seemed to exclude the possibility of matter holding together in chemical union against so great a separating energy. And matter in this primitive condition would be incapable of luminous radiation; so that a dark nucleus was provided for the background of a hole in the photosphere, to account for the spot. But here the theory broke down under Dr. Balfour Stewart's remark that the principle of reciprocity of radiation and absorption would claim for the non-radiating solar nucleus a transparency which must show the white photosphere at the opposite side of the Sun, in place of the dark background required for the spot. It failed therefore to account for the spot, and so lost its *raison d'etre*. But independently of extrinsic tests of fitness, the theory seems to overreach itself intrinsically. We cannot differentiate between heat rays and light rays in denying the property of radiation to dissociated matter; and we cannot reasonably admit the property of communicating heat by contact in a mass which is unable to give off heat by radiation. A thermometer, if it could be introduced into the solar nucleus without passing through the fiery photosphere, and could be completly screened from the photospheric radiation by a perfect reflector, would show no change of temperature; for its surroundings could neither give nor receive heat. In other words the dissociation of a fragment of matter, entering into the solar nucleus becomes impossible, for the maximum temperature that can be imparted to it is that of the photosphere, which by supposition is below the the temperature of dissociation; and reassociation is impossible, for dissociated matter can lose no heat.*

† The writer does not deny either that dissociation is possible, or that dissociated matter is incapable of radiation and absorption. Both may be accepted and also the consequences enumerated below, without violence to reason.

1°. All the heat of dissociated matter is absolutely latent: dissociated matter cannot part with heat, it has its fill and can receive no more, it can only transmit what comes to it.

Corollary 1. The temperature of dissociation is the maximum temperature of matter.

We have left out of consideration so far, the internal pressure of the Sun's mass. It has an important bearing upon the physical aspect of the solar nucleus, if not upon its physical structure. The pressure effect upon the spectrum of gaseous radiation is now known to be a widening of the radiation lines; and this to such an extent that, at the great pressure in the interior of the Sun, the radiation energy must have a continuous spectrum. So that we have in a manner to return to the statement of Kirchhoff that the solar nucleus is either a glowing rock or a molten sea of fire, in as much as it may be the equivalent of either, in its white radiation. This is our nearest direct experimental test of Faye's theory, and its witness is against the dark nucleus: for whatever be the properties of dissociated matter, if it is contained within a fluid shell it must be subject to pressure, and to exclude the spectrum effect of pressure could only be, what Miss Clerke has called in another case, "an arbitrary expedient of theory."

The same conclusion, although not opposed to the misty photosphere, claimed by the Kew school, is not in favor of it; for the incandescent cloud of condensation would be rather a screen to the more energetic radiation of nucleus than itself the light distributer. But has it a place in solar meteorology? We venture the answer,—no. When we try to measure the intensity of solar radiation within its own atmosphere, by comparison with the heat experienced at our distance of over 90,000,000 of miles, we are forced to put the question: is condensation possible amidst those untold heats; and the answer comes with irresistible force that no metallic substance known to us can assume the vapor state anywhere in or near the solar chromosphere unless perchance under the chilling influence of expansion in eruptive disturbances.

A third explanation of spot-formation is that of the late Father

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Corollary 2. The potential energy of dissociated matter is very great, responding to its great latent heat.

2°. Dissociated matter can neither exert nor suffer pressure; for pressure fails together with ability to communicate heat by contact.

Corollary 1. Dissociated matter cannot be confined by associated matter, but permeates all forms of chemical structures.

Corollary 2. The temperature of dissociation is another critical temperature, at which matter escapes the grasp of pressure, as the vaporization of ether eludes it in the experiment of Cagniard de la Tour.

3°. Re-association is impossible: for dissociated matter cannot lose heat.

Finally dissociated matter cannot be called primordial, but is a final state. Its properties are not those of the solid, the liquid, or the gaseous state of chemical matter; but they are apparently those of the ubiquitous caloriferous and luminiferous ether. It is imperishable. It has no temperature. Its high potential energy may account for the enormous elasticity indicated by the velocity of light. It may be subject to changes of potential, analogous to the pressure and tension of mechanics, resulting in the phenomena of electrical discharges.

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Secchi, communicated to Professor Newcomb's Popular Astronomy in 1877. In this paper the darkness is accounted for by the general absorption of the relatively cooler gases descending upon the photosphere, and this mode of stopping the light from the photosphere would be very acceptable as dispensing with the condensations which appear to us impossible, if it could be brought into harmony with the spectroscopic testimony upon the state of the gases. But the spectroscope bears witness against it. For general absorption begins under those conditions of pressure where selective absorption ceases; and the spectroscope shows that selective absorption in the line of sight to the spot is very pronounced. It is true that there is nothing in the nature of things to disprove the association of a feebler general absorption with a stronger selective absorption in gases under ordinary pressures. But the burthen of demonstration is on the other side; for selective and general absorption are opposed to one another, and what evidence we have from the spectroscope is against the admission of a general absorptive action by matter in the gaseous state. This evidence comes from the reciprocal action of radiation, in which we find the lines of specific radiation, corresponding to those of selective absorption, quite sharply defined, and consequently showing no increase of general radiation in the immediate neighborhood of its most active radiation; and we must suppose that if there were any general radiation by the incandescent gas, this would be stronger in the wave-lengths most nearly equal to those of its specific radiation.

M. Faye's latest theory, as set out in his graphic description of the solar whirlpools and cyclones, in "*l'origine du Monde*," brings us no nearer the goal of our enquiry—a satisfactory cause of spot-darkness. The funnel-shaped depression produced by the gyratory motion set up in the photosphere could give the required darkness only in two ways: it might sufficiently reduce the depth of the photosphere in the direction of our vision to result in a relative darkness, if the author's original theory of a dark solar nucleus could be admitted: it would certainly draw down into it relatively cooler hydrogen, as the author has it; but the cooler hydrogen could stop the light only by reflection, by refraction, or by absorption; and of these, absorption only could claim consideration; but if absorption could be accepted as the stopping agent, we should prefer Secchi's theory, on the grounds that the disturbing energy is more probably amongst the greater than amongst the lesser heats of the solar furnace.

So far therefore in our search amongst the prominent theories

of solar spot formation, we have failed to find a satisfactory cause of the darkness. Faye, in his original theory, struck at the root of the difficulty by removing the candle: he bored a hole through the luminous source, and so got rid of the light. The Kew school placed a cloud-screen of condensed vapors, cooled by radiation into space, to stop a part of the light; and Father Secchi adopted an absorption screen of the cooler descending gases. We have been unable to accept any one of these, and are left to seek the explanation either amongst the unknown capabilities of electrical action, or in the chilling effect of expansion already alluded to.

Electrical action has been suggested to more than one observer of the solar prominences. These fantastic and changing forms can hardly fail to recall the impression of electrical illuminations of rarified gases. But we are not at liberty to attribute effects to doubtful causes, until we have become in some way acquainted with their aptitude; and as yet we have no proof that electrical illumination is apt to exalt or degrade radiation, after the manner of an approaching or receding light-source, to account for the occasional distortions witnessed with the spectroscope. Electrical action increases the opacity of a cloud of minute condensations, and much of the darkening of a solar spot may be attributed to it, but Mr. Aitken's experiments* go far to show that the immediate effect of this action is not condensation, but a multiplication of smaller aqueous specks by the fracture of the larger drops: in other words that the immediate effect of electrical action is not darkness but an increased opaqueness of a screen already produced by another agent.

The alternative supposition is free from any imputation of impossibility, and has the strongest claims to represent the truth. We want a cooling source akin to Faraday's refrigerator when he succeeded in freezing mercury in a white hot crucible; and the chill of expansion seems to us the only possible agent of the kind in the big solar crucible. A solar eruption must owe all its developed kinetic energy to the pent up heat energy suddenly released by an external disturbance of the balance of pressure. The work done in expansion by lifting the superincumbent atmosphere would be great and all at the expense of heat in the expanding gases. In the lower levels of the outrush a cloud of metallic precipitations would form, stopping by reflection the photospheric radiation; while the uplifted masses would receive an increase of heat in the process, and would be placed at a position of advan-

‡ Proceedings R. Soc. LI, p. 408.

tage for radiation into space, less hindered by absorption. These would be to our telescopes as luminous clouds, and their gaseous state would be shown in our spectroscopes. They would necessarily drift away from the centre of the eruption and the dark cloud below would then be revealed to us as a solar spot surrounded with faculæ.

This genesis of the spot and its attendant faculæ is in complete harmony with the observed details of its structure, and of its birth and growth: the dark area is below the photosphere, the faculæ are above the photosphere, and it is highly probable that the preparatory sign of a new spot is always a small bright patch of facula as the cradle of its infancy.

It is generally admitted that spots are real cavities in the solar photosphere, although not all observers have been able to verify the foreshortening effect upon the penumbra described by the Glasgow professor, Dr. Wilson. But it is at least a reasonable supposition that the dark space descends below the luminous surface of the Sun. This would be the region of released pressure or of rarefaction consequent upon the expansive eruption,—the region of chill and of precipitations,—where the cooler gases would augment the selective absorption, and the cloud of dark dust would partially screen the white photospheric radiation.

It is no less certain that the faculæ are at higher elevations, and that many of them extend to great heights. We cannot place much reliance upon mere appearances, as of faculæ crossing over or even diving into the cavity. The same appearance could be produced by more intensely glowing parts of the photosphere below the spot. But there is reason for saying that solar prominences are faculæ viewed in elevation, and in this picture it is the proper function of the eye to assign relative position. All observers of the solar prominences know that the finest displays of these fiery forms are always to be expected when the faculæ of a spot are turning over the solar limb; and the photographic plates of Professor Hale and M. Deslandres and our own show the same spectrum of prominence and faculæ, so far as the latter can be observed. The reversals of the H and K lines photographed in prominences are even more easily obtained from the faculæ. The favorable condition of the prominence for the display of its brigt-line spectrum consists in the darker background off the edge of the photosphere, and the same contrast is afforded even more efficiently for the H and K lines of the spectrum of faculæ by the extensive calcium absorption in the solar atmosphere. Father Secchi has put on record a remarkable phenomenon

sketched by Tacchini on August 8, 1865; a luminous appendage to the Sun was seen as it was setting below the sea line of the Mediterranean, the appearance was that of a prominence; but its height greatly exceeded anything of the kind witnessed in the spectroscope. The time corresponded exactly to that of the arrival upon the solar limb of an extensive facula sketched at the Observatory of the Roman College on the previous day; and the two drawings are strikingly similar. There can be no doubt about the possibility of this very exceptional phenomenon. It only supposes that the condition of our atmosphere was, at the time, that of an excellent filtering cell for the hydrogen red light, all along the horizontal line of sight towards the setting Sun: a very exceptional condition of things, and only twice utilized so far as we learn from Father Secchi, once by Tacchini on the setting Sun in the Mediterranean, and once at the rising of the Sun at Poestum by an unnamed traveller.*

The third postulate for the acceptance of the proposed genesis of the spot, needs more consideration, in as much as it is not universally accepted as true. This is the precedence of faculæ to spot formation. It is not a necessary consequence of the process traced out; but the complete absence of the priority of faculæ would greatly weaken the claims we are advancing. It is possible that the extra brilliancy of the faculous covering of the spot, at its birth, should be so nearly balanced by the underneath darkening of the photosphere, as to present no apparent change from the normal surface-radiation; the loss of light may even be greater than its increment, when the spot would be the first to show itself; but we should expect that, sometimes at least, the bright radiant mass of uplifted gases should have the advantage, and shine as luminous clouds concealing the under darkness; and examples of this are not wanting. Two of these have been recorded in a note to the *Monthly Notices* R. A. S. for December, 1891,† and reprinted in ASTRONOMY AND ASTRO-PHYSICS for March, 1892; and four others have been added to these in the course of the examination of the Stonyhurst drawings of solar spots and faculæ in the minimum period of 1889. These have been mentioned in the report of the Observatory in the *Monthly Notices* for February, 1893.‡ It will suffice here to formulate the conclusion of our studies of the life histories of the spots of this period—that whereas no clear examples have been found of a

* Bull. Meteorol. Coll. Rom., 1867, p. 87.
† *Monthly Notices*, R. A. S., Vol. LII, p. 104.
‡ *Monthly Notices*, R. A. S., Vol. LIII, p. 251.

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spot appearing before faculæ, we have six undeniable spot births in the midst of small bright patches of faculæ which had formed within the one or two preceding days.

So far the prominent features of a solar spot are reasonably accounted for. There are other facts in the histories of their life-periods which need explanation. The long duration of the dark nucleus and still longer life of the faculæ present no difficulty when the magnitude of the disturbed masses is approximately realized. We are dealing with unknown masses, it is true, but we cannot doubt their greatness in the enormous volumes which they occupy. Months give time short enough for the re-establishment of heat equilibrium in regions so vast as these. But we have yet no clue to the cause of duplicate spots or to the apparent repulsive action between them. In all the greater disturbances there are two dark centres, and these continuously separate from one another until one of them fades away. There is too great uniformity in the pairs to admit the supposition of chance proximity; and their slow separation is too constant to leave any doubt about a physical relationship between the companions. We as naturally look to electricity, as the medical man turns to nerve centres, to account for seemingly unexplainable phenomena. No one will doubt that there must be great electrical disturbances in connection with spot-formation; and the separation of the two nuclei would point to the action of similar electrifications, if we had grounds for admitting an intensity equal to the effect at the immense distances between them. But electrification offers no sort of answer to the question why there should be two centres of disturbance not oppositely electrified.

STONYHURST COLLEGE OBSERVATORY.

ON THE THEORY OF STELLAR SCINTILLATION.*

LORD RAYLEIGH, SEC. R. S.

Arago's theory of this phenomenon is still perhaps the most familiar, although I believe it may be regarded as abandoned by the best authorities. According to it the momentary disappearance of the light of the star is due to accidental interference between the rays which pass the two halves of the pupil of the eye or the object-glass of the telescope. When the relative retardation amounts to an odd multiple of the half wave-length of any

* From *Philosophical Magazine*, July 1893.

kind of light, such light, it is argued, vanishes from the spectrum of the star. But this theory is based upon a complete misconception. "It is as far as possible from being true that a body emitting homogeneous light would disappear on merely covering half the aperture of vision with a half wave-plate. Such a conclusion would be in the face of the principle of energy, which teaches plainly that the retardation in question would leave the aggregate brightness unaltered."* It follows indeed from the principle of interference that there will be darkness at the precise point which before the introduction of the half wave-plate formed the centre of the image, but the light missing there is to be found in a slightly displaced position.†

The older view that the scintillation is due to the actual diversion of light from the aperture of vision by atmospheric irregularities was powerfully supported by Montigny,‡ to whom we owe also a leading feature of the true theory, that is the explanation of the chromatic effects by reference to the different paths pursued by rays of different colors in virtue of *regular* atmospheric dispersion. The path of the violet ray lies higher than that of the red ray which reaches the eye of the observer from the same star, and the separation may be sufficient to allow the one to escape the influence of an atmospheric irregularity which operates upon the other. In Montigny's view the diversion of the light is caused by total reflexion at strata of varying density.

But the most important work upon this subject is undoubtedly that of Respighi§, who, following in the steps of Montigny and Wolf, applied the spectroscope to the investigation of stellar

* *Enc. Brit.*, "Wave Theory," p. 441.

† Since the remarks in the text were written I have read the version of Arago's theory given by Mascart (*Traite d'Optique*, t. iii., p. 348). From this some of the most objectionable features have been eliminated. But there can be no doubt as to Arago's meaning. "Supposons que les rayons qui tombent à gauche du centre de l'objectif aient rencontré, depuis les limites supérieures de l'atmosphère, des couches qui, à cause de leur densité, de leur température, ou de leur état hygrométrique, étaient douées d'une réfringence différente de celle que possédaient les couches traversées par les rayons de droite; il pourra arrivér, qu'à raison de cette différence de réfringence, les rayons rouges de droit détruisent *en totalite* les rayons rouges de gauche, et que le foyer passe du blanc, son état normal, au vert; . . . . Voilà donc le résultat théorique parfaitement d'accord avec les observations; violà le phénomène de la scintillation dans une lunette rattaché d'une manière intime à la doctrine des interférences" (*l'Annuaire du Bureau des Longitudes pour* 1852, pp. 423, 425).

That the difference between Arago's theory and that followed in the present paper is fundamental will be recognized when it is noticed that, according to the former, the color effects of scintillation would be nearly independent of atmospheric *dispersion*. Arago gives an interesting summary of the views held by early writers.

‡ *Mem. de l'Acad. de Bruxelles*, t. xxviii. (1856).

§ *Roma, Atti Nuovi Lincei*, xxi (1868); *Assoc. Francaise, Compt. Rend.* i (1872) p. 169.

scintillation. The results of these observations are summed up under thirteen heads, which it will be convenient to give almost at full length.

(I.) In spectra of stars near the horizon we may observe dark or bright bands, transversal or perpendicular to the length of the spectrum, which more or less quickly travel from the red to the violet, or from the violet to the red, or oscillate from one to the other color; and this however the spectrum may be directed from the horizontal to the vertical.

(II.) In normal atmospheric conditions the motion of the bands proceeds regularly from red to violet for stars in the west, and from violet to red for stars in the east; while in the neighborhood of the meridian the movement is usually oscillatory, or even limited to one part of the spectrum.

(III.) In observing the horizontal spectra of stars more and more elevated above the horizon, the bands are seen sensibly parallel to one another, but more or less inclined to the axis of the spectrum, passing from red to violet or reversely according as the star is in the west or the east.

(IV). The inclination of the bands, or the angle formed by them with the axis (? transversal) of the spectrum depends upon the height of the star; it reduces to 0° at the horizon and increases rapidly with the altitude so as to reach 90° at an elevation of 30° or 40°, so that at this elevation the bands become longitudinal.

(V.) The inclination of the bands, reckoned downwards, is towards the more refrangible end of the spectrum.

(VI.) The bands are most marked and distinct when the altitude of the star is *least*. At an altitude of more than 40° the longitudinal bands are reduced to mere shaded streaks, and often can only be observed upon the spectrum as slight general variations of brightness.

(VII.) As the altitude increases, the movement of the bands becomes quicker and less regular.

(VIII.) As the prism is turned so as to bring the spectrum from the horizontal to the vertical position, the inclination of the bands to the transversal of the spectrum continually diminishes until it becomes zero when the spectrum is nearly vertical; but the bands then become less marked, retaining, however, the movement in the direction indicated above (III).

(IX.) Luminous bands are less frequent and less regular than dark bands, and occur well marked only in spectra of stars near the horizon.

(X.) In the midst of this general and violent movement of bright and dark masses in the spectra of stars, the black spectral lines proper to the light of each star remain sensibly quiescent or undergo very slight oscillations.

(XI.) Under abnormal atmospheric conditions the bands are fainter and less regular in shape and movement.

(XII.) When strong winds prevail the bands are usually rather faint and ill defined, and then the spectrum exhibits mere changes of brightness, even in the case of stars near the horizon.

(XIII.) Good definition and regular movement of the bands seems to be a sign of the probable continuance of fine weather, and, on the other hand, irregularity in these phenomena indicates probable change.

These results show plainly that the changes of intensity and color in the images of stars are produced by a momentary real diversion of the luminous rays from the object-glass of the telescope; that in the neighborhood of the horizon rays of different colors are affected separately and successively, and that all the rays of a given color are momentarily withdrawn from the whole of the object-glass.

Most of his conclusions from observation were readily explained by Respighi as due to irregular refractions, not necessarily or usually amounting (as Montigny supposed) to total reflections, taking place at a sufficient distance from the observer. The progress of the bands in one direction along the spectrum (II.) is attributed to the diurnal motion. In the case of a setting star, for instance, the blue rays by which it is seen, pursuing a higher course through the atmosphere, encounter an obstacle somewhat later than do the red rays. Hence the band travels toward the violet end of the spectrum. In the neighborhood of the meridian this cause of a progressive movement ceases to operate.

The observations recorded in (III.) are of special interest as establishing a connection between the rates with which various parts of the object-glass and of the spectrum are affected. Since the spectrum is horizontal, various parts of its width correspond to various horizontal sections of the objective, and the existence of bands at a definite inclination shows that at the moment when the shadow of the obstacle thrown by blue rays reaches the bottom of the glass the shadow at the top is that thrown by green, yellow or red rays of less refrangibility. When the altitude of the star reaches 30° or 40°, the difference of path due to atmospheric dispersion is insufficient to differentiate the various parts of the spectrum. The bands then appear longitudinal.

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The *definite* obliquity of the bands at moderate altitudes, reported by Respighi, leads to a conclusion of some interest, which does not appear to have been noticed. In the case of a given star, observed at a given altitude, the linear separation at the telescope of the shadows of the same obstacle thrown by rays of various colors will of necessity depend upon the distance of the obstacle. But the definiteness of the obliquity of the bands requires that this separation shall not vary, and therefore that the obstacles to which the effects are due are sensibly at one distance only. It would seem to follow from this that, under "normal atmospheric conditions," scintillation depends upon irregularities limited to a comparatively narrow horizontal stratum situated overhead. A further consequence will be that the distance of the obstacles increases as the altitude of the star diminishes, and this according to a definite law.

The principal object of the present communication is to exhibit some of the consequences of the theory of scintillation in a definite mathematical form. The investigation may be conducted by simple methods, if, as suffices for most purposes, we regard the whole refraction as small, and neglect the influence of the Earth's curvature. When the object is to calculate with accuracy the refraction itself further approximations are necessary, but even in this case the required result can be obtained with more ease than is generally supposed.

The foundation upon which it is most convenient to build is the idea of James Thomson,* which establishes instantaneously the connection between the curvature of a ray travelling in a medium of varying optical constitution and the rate at which the index changes at the point in question. The following is from Everett's memoir:

"Draw normal planes to a ray at two consecutive points of its path. Then the distance of their intersection from either point will be ρ, the radius of curvature. But these normal planes are tangential to the wave-front in its two consecutive positions. Hence it is easily shown by similar triangles that a very short line dN drawn from either of the points towards the centre of curvature is to the whole length ρ, of which it forms part, as dv the difference of the velocities of light at its two ends is to v the velocity at either end. That is,

$$dN/\rho = -dv/v,$$

the negative sign being used because the velocity diminishes in ap-

* Brit. Assoc. Rep. 1872. Everett *Phil. Mag.* March 1873.

proaching the centre of curvature. But since v varies inversely as μ, we have

$$-dv/v = d\mu/\mu.$$

Hence the curvature $1/\rho$ is given by any of the four following expressions:

$$\frac{1}{\rho} = -\frac{1}{v}\frac{dv}{dN} = -\frac{d\log v}{dN} = \frac{1}{\mu}\frac{d\mu}{dN} = \frac{d\log\mu}{dN} \qquad (1)$$

"The curvatures of different rays at the same point are directly as the rates of increase of μ in travelling along their respective normals." If θ denote the angle which the ray makes with the direction of most rapid increase of index, the curvatures will be directly as the values of $\sin\theta$. In fact, if $d\mu/dr$ denote the rate at which μ increases in a direction normal to the surfaces of equal index, we have

$$\frac{d\mu}{dN} = \frac{d\mu}{dr}\sin\theta,$$

and therefore

$$\pm\frac{1}{\rho} = \frac{1}{\mu}\frac{d\mu}{dr}\sin\theta = \frac{d\log\mu}{dr}\sin\theta. \qquad (2)$$

Everett shows how the well-known equation

$$\mu p = \text{const.} \qquad (3)$$

can be deduced from (2), p being the perpendicular upon the ray from the centre of *spherical* surfaces of equal index. In general,

$$\frac{1}{\rho} = \frac{1}{r}\frac{dp}{dr}, \quad \sin\theta = \frac{p}{r},$$

and thus

$$-\frac{1}{r}\frac{dp}{dr} = \frac{p}{r}\frac{d\log\mu}{dr},$$

giving (3) on integration.

At a first application of (2) we may find by means of it a first approximation to the law of atmospheric refraction, on the supposition that the whole refraction is small and that the curvature of the Earth may be neglected. Under these limitations θ in (2) may be treated as constant along the whole path of the ray; and if $d\psi$ be the angle through which the ray turns in describing the element of arc ds, we have

$$d\psi = \frac{d\log\mu}{dr}\sin\theta ds = \tan\theta\, d\log\mu.$$

If we integrate this along the whole course of the ray through

the atmosphere, that is from $\mu = 1$ to $\mu = \mu_0$, we get, as the whole refraction,

$$\psi = \log \mu_0 \tan \theta = (\mu_0 - 1)\tan \theta, \qquad (4)$$

for to the order of approximation in question $\log \mu_0$ may be identified with $(\mu_0 - 1)$.

If $\delta\psi$ denote the chromatic variation of ψ corresponding to $\delta\mu_0$, we have from (4)

$$\delta\psi/\psi = \delta\mu_0/(\mu_0 - 1). \qquad (5)$$

According to Mascart* the value of the right-hand member of (5) in the case of air and of the lines B and H is

$$\delta\mu_0/(\mu_0 - 1) = .024. \qquad (6)$$

We will now take a step further and calculate the linear deviation of a ray from a straight course, still upon the supposition that the whole refraction is small. If η denote the linear deviation (reckoned perpendicularly) at any point defined by the length s measured along the ray θ, we have

$$\frac{d^2\eta}{ds^2} = \frac{1}{\rho} = \tan\theta \frac{d\log\mu}{ds},$$

so that

$$\frac{d\eta}{ds} = \int \tan\theta \, d\log\mu = \tan\theta(\mu - 1) + \alpha,$$

α being a constant of integration. A second integration now gives

$$\eta = \tan\theta \int (\mu - 1)ds + \alpha s + \beta \qquad (7)$$

which determines the path of the ray. If y be the height of any point above the surface of the Earth, $ds = dy \sec\theta$; so that (7) may also be written

$$\eta = \frac{\sin\theta}{\cos^2\theta} \int (\mu - 1)dy + \alpha s + \beta \qquad (8)$$

The origin of s is arbitrary, but we may conveniently take it at the point (A) where the ray strikes the Earth's surface.

We will now consider also a second ray, of another color, deviating from the line θ by the distance $\eta + \delta\eta$ and corresponding to a change of μ to $\mu + \delta\mu$. The distance between the two rays at any point y is

$$\delta\eta = \frac{\sin\theta}{\cos^2\theta} \int_0^y \delta\mu dy + d\alpha \, . \, s + \delta\beta \qquad (9)$$

* Everett's C.G.S. System of Units.

In this equation $\delta\beta$ denotes the separation of the rays at A, where $y = 0$, $s = 0$. And $\delta\alpha$ denctes the angle between the rays when outside the atmosphere.

Equation (9) may be applied at once to Montigny's problem, that is to determine the separation of two rays of different colors, both coming from the same star, and both arriving at the same point A. The first condition gives $\delta\alpha = 0$, and the second gives $\delta\beta = 0$. Accordingly,

$$\delta\eta = \frac{\sin\theta}{\cos^2\theta}\int_0^y \delta\mu dy \qquad (10)$$

is the solution of the question.

The integral in (10) may be otherwise expressed by means of the principle that $(\mu - 1)$ and $\delta\mu$ are proportional to the density. Thus, if l denote the "height of the homogeneous atmosphere," and h the elevation in such an atmosphere determined by the condition that there shall be as much air below it as actually exists below y,

$$\int_0^y \delta\mu dy = \delta\mu_0 h \qquad (11)$$

$\delta\mu_0$ being the value of $\delta\mu$ at the surface of the Earth. Equation (10) thus becomes

$$\delta\eta = \frac{\delta\mu_0 h \sin\theta}{\cos^2\theta} \qquad (12)$$

At the limits of the atmosphere and beyond, $h = l$, and the separation there is

$$\delta\eta = \frac{\delta\mu_0 l \sin\theta}{\cos^2\theta} \qquad (13)$$

These results are applicable to all altitudes higher than about 10°.

The formulæ given by Montigny (*loc. cit.*) are quite different from the above. That corresponding to (13) is

$$\delta\eta = \delta\mu_0\, a \sin\theta, \qquad (14)$$

a being the radius of the Earth! The substitution of a for l increases the calculated result some 800 times. But this is in a large measure compensated by the factor $\sec^2\theta$ in (13), for at low altitudes sec θ is large. According to Montigny the separation at moderately low altitudes would be nearly independent of the altitude, a conclusion entirely wide of the truth.

The value of $(\mu_0 - 1)$ for air at 0° and 760 millim. at Paris is .0002927, so that $\delta\mu_0$ (for the lines B and H) is .000007025. The

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height of the homogeneous atmosphere is 7.990×10^5 centim., and thus $\delta\eta$ reckoned in centim. is

$$\delta\eta = 5.612 \frac{h \sin\theta}{l \cos^2\theta}. \qquad (15)$$

The following are a few corresponding values of θ and $\sin\theta \cos^2\theta$:

θ	$\sin\theta\cos^2\theta$	θ	$\sin\theta\cos^2\theta$
0°	0.000	60°	3.46
20	0.387	70	8.03
40	1.095	80	32.66

Thus at the limit of the atmosphere the separation of rays which reach the observer at an apparent altitude of 10° is 185 centim. Nearer the horizon the separation would be still greater, but its value cannot well be found from (15). Although these estimates are considerably less than those of Montigny, the separation near the horizon seems to be sufficient to explain the vertical position of the bands in the spectrum, recorded by Respighi (I). The fact that the margin is not very great suggests that the obstacles to which scintillation is due may often be situated at a considerable elevation.

We have now to consider the effect of an obstacle situated at a given point B at level y on the course of the ray. And the first desideratum will be the estimation of the separation at A, the object-glass of the telescope, of rays of various colors coming from the same star, which all pass through the given point B. It will appear at once that no fresh question is raised. For since the rays come from the same star at the same time, $\delta\alpha = 0$, and thus by (9) $\delta\eta_A = \delta\beta$. The value of $\delta\beta$ is given at once by the condition that $\delta\eta_B = 0$. Thus.

$$-\delta\eta_A = \frac{\sin\theta}{\cos^2\theta}\int_0^\infty \delta\mu dy = \frac{\delta\mu_0 h \sin\theta}{\cos^2\theta}, \qquad (16)$$

as before. The discussion, already given of (15), is thus immediately applicable.

Equation (16) solves the problem of determining the inclination of the bands seen in the spectra of stars not very low (III). It is only necessary to equate $-\delta\eta_A$ to the aperture of the telescope. $\delta\mu_0$ then gives the range of refrangibility covered by the bands as inclined. In practice h would not be known beforehand; but from the observed inclination of the bands it would be possible to determine it.

In a given state of the atmosphere h, so far as it is definite, must be constant and then $\delta\mu_0$ must be proportional to $\cos^2\theta/\sin\theta$.

This gives the relation between the altitude of the star and the inclination of the bands.

When θ is small, $\delta\mu_0$ is large; that is the bands become longitudinal.

As a numerical example, let us suppose that the aperture of the telescope is 10 centim., and that at an altitude of 10° the obliquity of the bands is such that the vertical diameter of the object-glass corresponds to the entire range from B to H. In this case (15) gives

$$h = \frac{10\,l}{185} = .054\,l,$$

indicating that the obstacles to which the bands are due are situated at such a level that about $\frac{1}{20}$ of the whole mass of the atmosphere is below them.

The next question to which (9) may be applied is to find the angle $\delta\alpha$ outside the atmosphere between two rays of different colors which pass through the *two* points A and B. Here $\delta\eta_A = 0$, and thus $\delta\beta = 0$. And further, since $\delta\eta_0 = 0$, we get

$$-\delta\alpha = \frac{\sin\theta}{s\cos^2\theta_s}\int_0^y \delta\mu\,dy = \frac{\delta\mu_0 h\tan\theta}{y} \qquad (17)$$

If the height of the obstacle above the ground be so small that the density of the air below it is sensibly uniform, then $h = y$, and

$$-\delta\alpha = \delta\mu_0\tan\theta. \qquad (18)$$

In this case the angle is the same as that of the spectrum of the star observed at A, as appears from (4) and (5). In general, y is greater than h, so that $\delta\alpha$ is somewhat less than the value given by (18).

The interest of (18) lies in the application of it to find the time occupied by a band in traversing the spectrum in virtue of the diurnal motion, according to Respighi's observation (II). The time required is that necessary for the star to rise or fall through the angle of its dispersion-spectrum at the altitude in question. At an altitude of 10°, this angle will be 8″, being always about $\frac{1}{40}$ of the whole refraction. The rate at which a star rises or falls depends of course upon the declination of the star and upon the latitude of the observer, and may vary from zero to 15° per hour. At the latter maximum rate the star would describe 8″ in about one half of a second, which would therefore be the time occupied by a band in crossing the spectrum under the circumstances supposed. In the case of a star quite close to the horizon, the progress of the band would be a good deal slower.

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The fact that the larger planets scintillate but little, even under favorable conditions, is readily explained by their sensible apparent magnitude. The separation of rays of one color thus arising during their passage through the atmosphere is usually far greater than the already calculated separation due to chromatic dispersion; so that if a fixed star of no apparent magnitude scintillates in colors, the different parts of the area of a planet must *à fortiori* scintillate independently. But under these circumstances the eye perceives only an average effect, and there is no scintillation visible.

The non-scintillation of small stars situated near the horizon may be referred to the failure of the eye to appreciate color when the light is faint.

In the case of stars higher up the whole spectrum is affected simultaneously. A momentary accession of illumination, due to the passage of an atmospheric irregularity, may thus render visible a star which on account of its faintness could not be steadily seen through an undisturbed atmosphere.*

In the preceding discussion the refracting obstacles have for the sake of brevity been spoken of as throwing sharp shadows. This of course cannot happen, if only in consequence of diffraction; and it is of some interest to inquire into the magnitude of the necessary diffusion. The theory of diffraction shows that even in the case of an opaque screen with a definite straight boundary, the transition of illumination at the edge of the shadow occupies a space such as $\sqrt{(b\lambda)}$, where λ is the wavelength of the light, and b is the distance across which the shadow is thrown. We may take λ at 6×10^{-5} centim., and if b be reckoned in kilometers, we have as the space of transition, $\sqrt{(6b)}$. Thus if b were 4 kilometers, the space of transition would amount to about 5 centim. The inference is that the various parts of the aperture of a small telescope cannot be very differently affected unless the obstacles to which the scintillation is due are at a less distance than 4 kilometres.

One of the principal outstanding difficulties in the theory of scintillation is to see how the transition from one index to another in an atmospheric irregularity can be sufficiently sudden. The fact that the various parts of a not too small object-glass are diversely affected seems to prove that the transitions in question do not occupy many centimetres. Now whether the irregularity be due to temperature or to moisture, we should expect

* The theory of Arago leads him to a directly opposite conclusion (*loc. cit.* p. 381).

that a transition, however abrupt at first, would after a few minutes or hours be eased off to a greater degree than would accord with the above estimate. Perhaps the abruptness of transition is, as it were, continually renewed by the coming into contact of fresh portions of light and dense air as the ascending and descending streams proceed in their courses. The speculations and experiments of Jevons on the Cirrus form of cloud* may find some application here. A preliminary question requiring attention is as to the origin of the irregularities which cause scintillation. Is it always at the ground, and mainly under the influence of sunshine? Or may irregular absorption of solar heat in the atmosphere, due to varying proportions of moisture, give rise to transitions of the necessary abruptness? Again, we may ask how many obstacles are to be supposed operative upon the same ray? Is the ultimate effect only a small residue from many causes in the main neutralizing one another? It does not appear that in the present state of meteorological science satisfactory answers can be given to these questions.

[TO BE CONTINUED.]

ASTRO-PHYSICAL NOTES.

All articles and correspondence relating to spectroscopy and other subjects properly included in ASTRO-PHYSICS, should be addressed to George E. Hale, Kenwood Observatory of the University of Chicago, Chicago, U. S. A. Authors of papers are requested to refer to last page for information in regard to illustrations, reprint copies, etc.

M. Janssen's Spectroscopic Observations on Mont Blanc.—In the *Comptes Rendus* of Sept. 25, M. Janssen describes his recent ascent of Mont Blanc, where a small observatory has been so nearly completed as to permit of its occupancy. In continuation of his well-known investigations on the absorption spectrum of oxygen he observed the B group in the solar spectrum with a much more powerful spectroscope than that used in his earlier work. This instrument has a Rowland grating and telescopes of 75 cm. focal length, and gives the B group in all its details. The employment of this instrument is a matter of some importance, as the spectroscope formerly used was not capable of resolving the group. The doublets which constitute the B group decrease in intensity as their wave-length increases, and the fainter doublets disappear successively as the absorptive action of the atmosphere diminishes. At the sea-level there are (according to M. Janssen) 13 or 14 doublets in addition to the "head" of the group. At Chamonix (1050 metres) the thirteenth doublet is seen with difficulty. At the Grands-Mulets (3050 metres) only 11 or 12 doublets are visible, while on the summit of Mont Blanc the number is reduced to 8.

* Phil. Mag. xiv. p. 22, 1857. For a mathematical investigation, by the author, see Math. Soc. Proc. xiv, April, 1883.

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M. Janssen finds that if the co-efficient which represents the diminution of atmospheric pressure at the summit of Mont Blanc

$$\left(\frac{0.43}{0.76} = 0.566\right)$$

is multiplied by the number 13 representing the doublets ordinarily clearly visible at the sea-level, the result is 7.4, or very nearly the number (8) of doublets visible at the summit of the mountain. But M. Janssen regards the law of diminution of doublets as in reality much more complex, and he has undertaken to reproduce at Meudon the phenomena observed at various altitudes, by means of tubes filled with oxygen, which can be subjected to any desired pressure. Preliminary experiments have led to the conclusion that the groups A, B and α would disappear from the solar spectrum at the limit of the atmosphere. In order to test whether the absorptive power of oxygen may not be of a peculiar character in the Sun on account of its high temperature M. Janssen has observed the absorptive spectra of the gas at temperatures of from 400° to 500°, but without detecting any appreciable modification.

A New Edition of Miss Clerke's History of Astronomy.*—"The interval of six years since the publication of the Second Edition of the present work has been marvellously fruitful of astronomical discoveries. Hence a searching revision was called for, and has been executed without stint of care or pains. Additions and substitutions have been freely made; and it has been the aim of the writer not only to furnish a large amount of requisite new information, but to incorporate it so completely with the pre-existing text as to leave no gaps in the narrative suggesting "interpolations" to the refined critical sense." These words, from the Preface to the Third Edition of Miss Agnes M. Clerke's well-known *History of Astronomy during the Nineteenth Century*, indicate the purpose of the new edition, and Miss Clerke's former readers will not be surprised to learn that this purpose has been most admirably fulfilled. The book reads like a romance, yet it deals with the present century's contributions to astronomy in no sensational way. That it has been thoroughly brought down to date may be evidenced by the fact that the two eclipses of 1889, the coronal theories of Professors Bigelow and Schaeberle, the most recent spectroscopic and photographic investigations of the Sun, the variation of latitude, the fifth satellite of Jupiter, and the remarkable history of Nova Aurigæ, have been given ample recognition.

The addition of five excellent photogravure plates, made from photographs of the Solar Chromosphere and Prominences, the Great Comet of September 1882, Swift's Comet, the Spectrum of Nova Aurigæ and the Milky Way in Sagittarius much increases the value of the work over former editions.

To every student of astronomy, and especially to the rapidly increasing number whose chief interest is in the progress of astro-physical investigation, we heartily recommend this book.

On the Absorption Spectra of Copper Salts.—The absorption spectra of aqueous solutions of copper chloride, sulphate and nitrate have been examined by Ewan, using Krüss's universal spectrometer for the purpose, and employing Vierordt's method. The conclusions reached in the investigation may be summed up as follows: (1). On diluting their solutions the absorption spectra of all the three salts examined are observed to change; (2) the character of these changes

* Macmillan & Co., New York.

is such as to show that the spectra in dilute solutions tend to become identical; (3) the behavior of the salts examined leads to the conclusion that in strong solutions the acid and basic parts of the salts are associated in producing absorption of light, whilst in dilute solutions they act independently in doing so; (4) these results are substantially in agreement with the hypothesis of electrolytic dissociation; (5) and finally the results obtained do not seem to be satisfactorily explicable upon the hypothesis of a hydrolytic dissociation or on that of molecular aggregates.—(*Am. Jour. Sci.*, October, 1893.)

Polarization of Undiffracted Ultra Red Radiations by Wire Gratings.—Du Bois and Rubens. (*Wied. Ann*, Bd. 49, p. 593, 1893.)—Some recent experiments by duBois show that polarized light passing directly (without diffraction) through a wire grating has its plane of polarization rotated. By the rotation the plane of polarization is made more nearly perpendicular to the direction of the wires of the grating; and this effect was explained by assuming that the grating was more transparent to light polarized perpendicular to the wires than to that parallel to them.

Since Troughton has shown* that the electrical vibration in polarized light must be considered as perpendicular to the plane of polarization, this result is contrary to the fact found by Hertz, viz., that a wire grating is transparent to electrical vibrations perpendicular to the direction of the wires but perfectly opaque to those parallel to the wires.

This want of agreement led to the further researches by du Bois and Rubens, here considered, on the relative transparency of fine wire gratings for light polarized parallel and perpendicular to the wires. To approach as near as possible to Hertz conditions the gratings were as fine as possible and the wave-lengths as great as could be used. Wires for the purpose were drawn of Pt, Cu, Fe, Au and Ag of diameters ranging from .0025 cm. to .006 cm. Two of these of the same diameter were wound on a frame and one then unwound, leaving a grating in which the transparent and opaque portions were equal. The light of a Zirconium burner or arc light was polarized by reflection from a pile of glass plates, sent through the grating, and its intensity measured by a spectrophotometer whose cross hair was represented by the wire of a bolometer. The grating was so mounted that it could be readily rotated 90° about an axis parallel to the direction of the light and thus its relative transparency in the two positions measured for the particular wave-length for which the bolometer wire was adjusted.

If we call n^2 the ratio of transparency for light polarized perpendicular to that polarized parallel to the grating wires, and draw a curve with the values of n^2 for ordinates and the corresponding wave-lengths for abscissæ, this curve will rise to a maximum in the ultra red and for still greater wave-lengths will cross the line $n^2 = 1$. For wave-lengths beyond this point the grating is more transparent for light polarized parallel to the wires; agreeing in this region with the results of Hertz.

The values of the wave-lengths for which n^2 is a maximum and for which it is unity depend on the material of the grating but not upon the size of the wires or of the spaces. For the longest wave-lengths n^2 approaches the same value for all the gratings and for the oscillations used by Hertz there would doubtless be no difference between copper and iron wires.

The results of the previous experiments of du Bois and of Hertz are thus

* *Phil. Mag.*, Vol. 32, p. 80, (1891).

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linked together and the present researches must be considered as affording a new and important confirmation to the identity of light waves and the electrical oscillations of Hertz.

In explanation of the facts it is suggested that there are probably two actions, one tending to weaken the vibrations perpendicular to the wires, the other weakening those at right angles to this. So long as the wave-length is very small compared with the size of the wires, the former effect exceeds the latter while for greater wave-lengths the reverse is true. For the short wave-lengths it is likely that the action is closely associated with the "Gouy-wien" phenomenon. For greater values of the wave-length it would appear (as in Hertz' experiments) that the freedom of electrical motion in the direction of the wires produces a greater opacity to vibrations in this direction.

The experimenters have also investigated the accuracy of the formula used by Sirks for the effect of wire screens on the brilliance of the image of a star. For the fine gratings used here a departure from the law was found, but assuming a law of deviation pointed out by Lorentz and Sande-Bakhuyzen, the error in the gratings used by Sirks amounts to but 0.03 magnitudes in the determination of the brilliance of the star. C. A. PERKINS.

Erratum in No. 118, October, 1893.

WASHINGTON, D. C., Oct. 10, 1893.

THE EDITOR OF ASTRONOMY AND ASTRO-PHYSICS:

In the foot note to the table of "deflecting forces" in article on "The Two Magnetic Fields surrounding the Sun," make it read, s_1 *units of 5th decimal,* instead of s_1 units of 6th decimal, to conform to the statement in the text, page 710, line 10.

Very respectfully,

FRANK H. BIGELOW.

Astronomical Journal Prizes.—By the last *Astronomical Journal* (305) it is stated that the judges for the award of the *Journal* prizes suggest that the period specified in the offer of the prize for the observations of comets found on page 160 of No. 1, Vol. XII, be extended by six months, to Sept. 30, 1894, for the following reasons:

"During the past six months but one comet has been discovered, and the period of its visibility was unusually short. Meanwhile winter is approaching during which the usually prevailing climatic conditions are likely to reduce chances for observation, even if the supply of new comets should not be restricted, as is usual, for the same reason. It is therefore probable, that from causes beyond the control of the observers, there will be adequate opportunity for worthy competition for so generous a prize.

To meet this unforeseen state of things by a measure in the interests of the candidates for the award, and fair to all of them alike, the above extension of time is proposed. The judges courteously recommend that, if the suggestion meets the approval of the donor, the terms of the offer should be modified in accordance therewith.

The donor of the prizes has assented to the change of conditions above named.

CURRENT CELESTIAL PHENOMENA.

PLANET NOTES FOR DECEMBER.

Mercury will be morning planet during December and will be visible to the unaided eye during the middle of the month. One must look toward the southeast about an hour before sunrise in order to see it. Mercury will be at greatest elongation, west from the Sun 21° 23′, Dec. 14 at noon.

Venus will be evening planet during December setting in the southwest between seven and eight P. M. Although so brilliant to the eye it will not, on account of its low altitude, be in good position for telescopic observation in northern latitudes. Venus will be at greatest elongation, east from the Sun 47° 29′, Dec. 6 at $3^h\ 36^m$ P. M. In the southern hemisphere this will be a very favorable opportunity to study the surface markings of Venus and it is to be hoped that Professor W. H. Pickering and his assistants at Arequipa will be able to add much to our knowledge of this subject and of the rotation of the planet.

Mars will be morning planet, but is getting farther south all the time so that its position will be unfavorable for northern observers. In the southern hemisphere the conditions will be much better. There will be quite a close conjunction of *Mars* and *Uranus* Dec. 6 at $4^h\ 09^m$ central time, when the former will be only 8′ north of the latter. Observers in Australia and Japan should be able to see the two planets in the same field of view of the telescope. The ruddy color of Mars and the green hue of Uranus will present a striking contrast. Eighteen hours later Mars will pass close to the wide double star α Libræ, the components of which Webb puts as third magnitude, pale yellow, and sixth magnitude light grey. Mars will pass 11′ north of the brighter star.

Jupiter, having but just passed opposition, will be in excellent position for observation during December. We have had a few good views of the planet this year when much of fine detail was seen upon the surface, notably a large number of very small dark red spots. We have not happened to look at the time when the "great red spot" was visible and cannot say what its appearance this year is. The apparent diameter of Jupiter during December diminishes from 46″ to 44″. His brilliancy will be greater than that of any other object in the evening sky, excepting the Moon, so that none can mistake him. His course is slowly westward in Taurus.

Saturn will be visible in the morning, but at a low altitude, so that for northern observers there will be no satisfactory observations. Saturn is in the constellation Virgo just a little north and east of the star Spica. The planet is the brighter of the two. The rings of Saturn are pretty well opened now, the angle of their plane to the line of sight being now about 12°, and increasing to 14° at the end of December. Saturn and the Moon will be in conjunction Dec. 3 at 3^h 20^m P. M. and Dec. 31 at $1^h\ 41^m$ A. M. Saturn will be about 3° north of the Moon in both instances.

Uranus is in Libra very close to the star α, referred to above in the note on Mars. At $5^h\ 32^m$ on the morning of Dec. 16, Uranus will be in conjunction with the star, only 3′ north. The conjunction with Mars has already been mentioned.

Neptune will be at opposition Dec. 3 and therefore in best position for observation during December. Its motion during the month will be 53′ west and 6′ south. The position Dec. 1 will be one third of the distance on a straight line from ι to ε Tauri. A photograph taken at Goodsell Observatory, Oct. 18, shows no star as bright as Neptune within 1° of this position.

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PLANET TABLES FOR DECEMBER.

[The times given are local time for Northfield. To obtain Standard Times for Places in approximately the same latitude, add the difference between Standard and Local Time if west of the Standard Meridian or subtract if east].

MERCURY.

Date. 1893.	R. A. h m	Decl. ° ′	Rises. h m	Transits. h m	Sets. h m
Dec. 5	15 40.2	− 16 47	5 47 A. M.	10 41.3 A. M.	3 36 P. M.
15	16 04.5	− 18 38	5 40 "	10 26.3 "	3 13 "
25	16 56.8	− 21 50	6 08 "	10 39.1 "	3 10 "

VENUS

Dec. 5	20 16.5	− 22 24	10 48 A. M.	3 17.0 P. M.	7 46 P. M.
15	20 56.8	− 19 20	10 35 "	3 17.7 "	8 01 "
25	21 31.5	− 15 47	10 14 "	3 13.0 "	8 12 "

MARS.

Dec. 5	14 39.7	− 15 00	4 38 A. M.	9 41.0 A. M.	2 44 P. M.
15	15 06.1	− 17 01	4 34 "	9 28.2 "	2 22 "
25	15 33.4	− 18 50	4 31 "	9 16.1 "	2 01 "

JUPITER.

Dec. 5	3 27.8	+ 17 46	3 10 P. M.	10 27.0 P. M.	5 44 A. M.
15	3 23 4	+ 17 32	2 27 "	9 43.3 "	4 59 "
25	3 19.9	+ 17 22	1 45 "	9 00.6 "	4 16 "

SATURN.

Dec. 5	13 25.9	− 6 30	2 50 A. M.	8 27.4 A. M.	2 05 P. M.
15	13 29.1	− 6 47	2 15 "	7 51.4 "	1 28 "
25	13 31.8	− 7 11	1 39 "	7 14.8 "	12 50 "

URANUS.

Dec. 5	14 42.7	− 15 22	4 43 A. M.	9 44.1 A. M.	2 45 P. M.
15	14 44.8	− 15 32	4 07 "	9 06.8 "	2 07 "
25	14 46.7	− 15 40	3 30 "	8 29.5 "	1 29 "

NEPTUNE.

Dec. 5	4 43.2	+ 20 42	4 11 P. M.	11 42.2 P. M.	7 13 A. M.
15	4 42.0	+ 20 40	3 31 "	11 01.7 "	6 32 "
25	4 40.9	+ 20 38	2 51 "	10 21.3 "	5 52 "

THE SUN.

Dec. 5	16 50.1	− 22 29	7 23 A. M.	11 51.0 A. M.	4 19 P. M.
15	17 34.1	− 23 19	7 32 "	11 55.6 "	4 20 "
25	18 18.5	− 23 23	7 37 "	12 00.6 "	4 24 "

Configuration of Jupiter's Satellites at 9:30 p. m. Central Time, for an Inverting Telescope.

Dec.		Dec.		Dec.	
1	2 3 ○ 1 4	12	3 2 ○ 1 4	23	4 1 ○ 2 3
2	1 4 ○ 2 3	13	3 1 ○ 2 4	24	4 ○ 1 2 3
3	4 ○ 2 1 3	14	3 2 1 ○ 4	25	4 2 1 ○ 3
4	4 2 1 ○ 3	15	2 3 ○ 1 4	26	4 2 ○ 3 1
5	4 3 ○ 1 ●	16	1 ○ 2 3 4	27	3 1 ○ 4 2
6	4 3 ○ 2 ●	17	○ 2 1 4 3	28	2 3 ○ 1 4
7	4 3 2 1 ○	18	2 1 ○ 4 3	29	3 2 ○ 4 ●
8	4 2 3 ○ 1	19	4 2 ○ 1 2	30	1 ○ 2 3 4
9	4 1 ○ 2 3	20	4 3 1 ○ 2	31	○ 1 2 3 4
10	● ○ 2 1 3	21	4 3 ○ 2 2		
11	2 1 ○ 3 4	22	4 2 3 ○ 1		

Elongations of the Satellites of Saturn.

(The western elongations will be found approximately half way between the eastern and other positions may be easily interpolated.)

MIMAS

		h		
Dec.	9	4.9	A. M.	E
	10	3.5	"	E
	11	2.1	"	E
	12	12.7	"	E
	12	11.3	P. M.	E
	13	9 9	"	E
	14	8.6	"	E
	15	7.3	"	E
	16	5.9	"	E
	17	4.5	"	E
	18	3.1	"	E
	19	1.7	"	E
	20	12.3	"	E
	21	10.9	A. M.	E
	22	9.5	"	E
	23	8.1	"	E
	24	6.8	"	E
	25	5.5	"	E
	26	4.1	"	E
	27	2.7	"	E
	28	1.3	"	E
	28	11.9	P. M.	E
	29	10.5	"	E
	30	9.1	"	E
	31	7.8	"	E

ENCELADUS

		h		
Dec.	15	6.6	A. M.	E
	16	3.5	P. M.	E
	18	12.4	A. M.	E
	19	9.3	"	E
	20	6.2	P. M.	E
	22	3.1	A. M.	E
	23	11.9	"	E
	24	8.8	P. M.	E
	26	5.7	A. M.	E
	27	2.6	P. M.	E
	28	11.5	"	E
	30	8.4	A. M.	E
	31	5.3	P. M.	E

TETHYS

		h		
Dec.	13	5.3	A. M.	E
	15	2.6	"	E
	16	11.9	P. M.	E
	18	9.3	"	E
	20	6.6	"	E
	22	3.9	"	E
	24	1.2	"	E
	26	10.5	A. M.	E
	28	7.8	"	E
	30	5.1	"	E

DIONE

		h		
Dec.	14	8.8	A. M.	E
	17	2.5	"	E
	19	8.2	P. M.	E
	22	1.9	"	E
	25	7.6	A. M.	E
	28	1.3	"	E
	30	7.0	P. M.	E

RHEA

		h		
Dec.	6	1.8	P. M.	E
	11	2.4	A. M.	E
	15	2.9	P. M.	E
	20	3.4	A. M.	E
	24	3.9	P. M.	E
	29	4.4	A. M.	E

TITAN

		h		
Dec.	14	6.9	P. M.	W
	22	4.3	"	E
	30	6.1	"	W

HYPERION

		h		
Dec.	15	8.4	P. M.	E
	27	2.2	"	W

JAPETUS

		h		
Dec.	10	6.2	A. M.	W
	30	4.5	P. M.	S

Occultations Visible at Washington.

Date 1893.	Star's Name.	Magnitude.	IMMERSION Washington M. T.	IMMERSION Angle f'm N pt.	EMERSION Washington M. T.	EMERSION Angle f'm N pt.	Duration.
			h m	°	h m	°	h m
Dec. 10	B.A.C. 6628	5.9	5 21	110	6 25	211	1 04
17	ζ Piscium	4.8	13 18	99	14 04	213	0 56
19	40 Arietis	6.3	6 32	359	7 08	294	0 36
20	36 Tauri	6.0	14 12	97	15 11	245	0 59
23	47 Geminorum	6.0	5 43	102	6 31	254	0 48
24	λ Cancri	5.7	6 43	106	7 33	266	0 50
30	86 Virginis	5.9	16 57	89	17 59	351	1 02

Phases and Aspects of the Moon.

		Central Time. d	h m	
New Moon	Dec.	8	1 40	A. M.
Apogee	"	9	5 24	A. M.
First Quarter	"	16	4 21	A. M.
Perigee	"	22	9 18	P. M.
Full Moon	"	22	10 37	P. M.
Last Quarter	"	29	5 18	P. M.

Phenomena of Jupiter's Satellites.

	h	m					h	m			
Dec.	2	1	48	A. M.	III	Oc. Dis.	Dec. 16	3	18	P. M.	II	Ec. Re.
		4	46	"	III	Ec. Re.		3	35	"	I	Tr. Eg.
	4	1	34	"	II	Tr. In.		4	18	"	I	Sh. Eg.
		2	20	"	II	Sh. In.	19	10	26	"	III	Tr. In.
		3	51	"	II	Tr. Eg.		11	53	"	III	Tr. Eg.
		4	20	"	I	Tr. In.	20	12	46	A. M.	II	Oc. Dis.
		4	41	"	II	Sh. Eg.		1	18	"	III	Sh. In.
		4	44	"	I	Sh. In.		2	16	"	I	Tr. In.
	5	1	31	"	I	Oc. Dis.		3	03	"	I	Sh. In.
		4	07	"	I	Ec. Re.		3	08	"	III	Sh. Eg.
		3	37	P. M.	III	Tr. In.		11	29	P. M.	I	Oc. Dis.
		5	06	"	III	Tr. Eg.	21	2	27	A. M.	I	Ec. Re.
		5	17	"	III	Sh. In.		7	21	P. M.	II	Tr. In.
		7	06	"	III	Sh. Eg.		8	43	"	I	Tr. In.
		8	15	"	II	Oc. Dis.		8	56	"	II	Sh. In.
		10	46	"	I	Tr. In.		9	31	"	I	Sh. In.
		11	13	"	I	Sh. In.		9	41	"	II	Tr. Eg.
		11	25	"	II	Ec. Re.		10	55	"	I	Tr. Eg.
	6	12	57	A. M.	I	Tr. Eg.		11	17	"	II	Sh. Eg.
		1	25	"	I	Sh. Eg.		11	44	"	I	Sh. Eg.
		7	57	P. M.	I	Oc. Dis.	22	5	56	"	I	Oc. Dis.
		10	36	"	I	Ec. Re.		8	56	"	I	Ec. Re.
	7	3	39	"	II	Sh. In.	23	1	56	"	II	Oc. Dis.
		5	00	"	II	Tr. Eg.		3	10	"	I	Tr. In.
		5	12	"	I	Tr. In.		3	15	"	III	Ec. Dis.
		5	41	"	I	Sh. In.		4	00	"	I	Sh. In.
		6	00	"	II	Sh. Eg.		4	52	"	III	Ec. Re.
		7	23	"	I	Tr. Eg.		5	22	"	I	Tr. Eg.
		7	54	"	I	Sh. Eg.		5	54	"	II	Ec. Re.
	8	5	05	"	I	Ec. Re.		6	13	"	I	Sh. Eg.
	11	3	51	A. M.	II	Tr. In.	24	3	25	"	I	Ec. Re.
		4	58	"	II	Sh. In.	27	1	40	A. M.	III	Tr. In.
	12	3	16	"	I	Oc. Dis.	28	1	16	"	I	Oc. Dis.
		6	55	P. M.	III	Tr. In.		9	43	P. M.	II	Tr. In.
		8	28	"	III	Tr. Eg.		10	30	"	I	Tr. In.
		9	18	"	III	Sh. In.		11	26	"	I	Sh. In.
		10	30	"	II	Oc. Dis.		11	35	"	II	Sh. In.
		11	07	"	III	Sh. Eg.	29	12	03	A. M.	II	Tr. In.
	13	12	31	A. M.	I	Tr. In.		12	42	"	I	Tr. Eg.
		1	08	"	I	Sh. In.		1	39	"	I	Sh. Eg.
		2	00	"	II	Ec. Re.		1	56	"	II	Sh. Eg.
		2	42	"	I	Tr. Eg.		7	43	P. M.	I	Oc. Dis.
		3	20	"	I	Sh. Eg.		10	52	"	I	Ec. Re.
		9	42	P. M.	I	Oc. Dis.	30	3	15	"	III	Oc. Dis.
	14	12	31	A. M.	I	Ec. Re.		4	16	"	II	Oc. Dis.
		5	00	P. M.	II	Tr. In.		4	57	"	I	Tr. In.
		6	18	"	II	Sh. In.		5	00	"	III	Oc. Re.
		6	57	"	I	Tr. In.		5	55	"	I	Sh. In.
		7	18	"	II	Tr. Eg.		7	09	"	I	Tr. Eg.
		7	37	"	I	Sh. In.		7	17	"	III	Ec. Dis.
		8	39	"	II	Sh. Eg.		8	08	"	I	Sh. Eg.
		9	09	"	I	Tr. Eg.		8	29	"	II	Ec. Re.
		9	49	"	I	Sh. Eg.		8	54	"	III	Ec. Re.
	15	4	09	"	I	Oc. Dis.	31	2	11	"	I	Oc. Dis.
		7	00	"	I	Ec. Re.		5	20	"	I	Ec. Re.

In. denotes ingress; Eg. egress; Dis., disappearance; Re., reappearance; Ec., eclipse; Oc., occultation; Tr., transit of the satellite; Sh., transit of the shadow. Only the phenomena visible in the United States are given. The times are Central Standard.

Minima of Variable Stars of the Algol Type.

[Given to the nearest hour in Central Standard Time.]

U CEPHEI.

R. A.....0h 52m 32s
Decl.....+81° 17′
Period.....2*d* 11h 50m

Dec. 6	3 A. M.
11	2 "
16	2 "
21	2 "
26	1 "
31	1 "

ALGOL.

R. A.....3h 1m 1s
Decl.....+ 40° 32′
Period.....2*d* 20h 49m

Dec. 10	4 A. M.
13	1 "
15	9 P. M.
18	6 "
30	6 A. M.

λ TAURI.

R. A..... ....3h 54m 35s
Decl.....+ 12° 11′
Period.....3*d* 22h 52m

Dec. 4	4 P. M.
8	3 P. M.

R. CANIS MAJORIS.

R. A..... 7h 14m 30s
Decl..... .....— 16° 11′
Period..... ..1*d* 3h 16m

Dec. 6	5 P. M.
7	8 "
8	11 "
10	3 A. M.
11	6 "
14	4 P. M.
15	8 "
16	11 "
18	2 A. M.
19	6 "

R. CANIS MAJ. CONT.

Dec. 23	6 P. M.
24	10 "
26	1 A. M.
27	4 "

S CANCRI.

R. A..... 8h 37m 39s
Decl.....+ 19° 26′
Period.....9*d* 11h 38m

Dec. 1	5 P. M.
10	5 A. M.
19	5 P. M.
29	4 A. M.

S ANTLIÆ.

R. A..... 9h 27m 30s
Decl.....— 28° 09′
Period..... 0*d* 7h 47m

Dec. 1	10 P. M.
2	6 A. M.
	9 P. M.
3	5 A. M.
	9 P. M.
4	5 A. M.
	8 P. M.
5	4 A. M.
	7 P. M.
6	3 A. M.
	6 P. M.
7	3 A. M.
	6 P. M.
8	2 A. M.
9	1 "
	midn.
10	midn.
11	11 P. M.
12	10 "
13	6 A. M.
	9 P. M.
14	6 A. M.

S. ANTLIÆ CONT.

Dec. 14	9 P. M.
15	5 A. M.
	8 P. M.
16	4 A. M.
	7 P. M.
17	4 A. M.
	7 P. M.
18	3 A. M
	6 P. M.
19	3 A. M.
	6 P. M.
20	2 A. M.
21	1 A. M.
22	1 A. M.
	midn.
23	midn.
24	11 P. M.
25	10 "
26	6 A. M.
	9 P. M.
27	6 A. M.
	8 P. M.
28	5 A. M.
	8 P. M.
29	4 A. M.
	7 P. M.
30	3 A. M.
	6 P. M.
31	3 A. M.
	6 P. M.

U. CORONÆ.

R. A 15h 13m 43s
Decl..... + 32° 03′
Period..... 3*d* 10h 51m

Dec. 11	5 A. M.
18	3 "
24	midn.

COMET NOTES.

We give below Mr. Brooks' account of the discovery of a new comet on the morning of Oct. 17. It was announced in the usual way by telegraph on that date. We have not been able to see the comet at Northfield on account of bad sky and the increasing moonlight, but hope that it has been observed sufficiently elsewhere to enable us to compute a rough orbit and find the comet when it comes out of the moonlight. A newspaper paragraph states that Mr. Brooks observed the comet on the morning of Oct. 22, and found it brighter than at the time of discovery. The tail could be traced to a distance of three degrees from the head. The position of the comet was given as R. A. 12h 20m, Decl. +16° 12′, which would indicate that the comet was moving almost due north.

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Discovery of Comet *c* 1893 (Brooks).—While sweeping the Eastern heavens this morning, which were unusually clear, it was my pleasure to discover a new telescopic comet. Position Oct. 16th, 16^h St. M. T. R. A. $12^h\ 21^m$, Decl. north $12°\ 55'$. Motion slow in a northeasterly course. The comet is bright in the 10-inch equatorial and has a short tail. WILLIAM R. BROOKS.

Smith Observatory, Geneva, N. Y., Oct. 17, 1893.

Elements and Ephemeris of Comet 1893 II (Rordame-Quennisset).—From *Astr. Nach.* No. 3192 we take the following elements and ephemeris by Professor V. Cerulli of Teramo, Italy. The orbit does not differ perceptibly from a parabola. It is doubtful whether the comet can be seen in the morning twilight, yet it is possible that some keen-eyed observer may find it again.

ELEMENTS.

$T = 1893$ July 7.31234 Berlin M. T.
$\omega = 47° 07' 37.0''$
$\Omega = 337\ 20\ 23.6$ } 1893.0
$i = 159\ 57\ 58.3$
$\log q = 9.829020$

EPHEMERIS.

Berlin Midnight.	App. R. A.			App. Decl.			log Δ	Aberration.	
	h	m	s	°	′	″		m	s
Nov. 1	12	45	32.03	+ 0	38	30.4	0.4715	24	37
3		45	53.06		32	26.8			
5		46	11.77		26	44.8	0.4727	24	41
7		46	28.01		21	24.9			
9		46	41.65		16	27.8	0.4732	24	43
11		46	52.56		11	54.0			
13		47	00.59		07	44.0	0.4730	24	42
15		47	05.60		03	58.2			
17		47	07.45	+ 0	00	37.2	0.4722	24	39
19		47	06.01	− 0	02	18.4			
21		47	01.14		04	48.0	0.4708	24	34
23		46	52.71		06	51.3			
25		46	40.55		08	27.4	0.4687	24	27
27		46	24.50		09	35.6			
29	12	46	04.41	− 0	10	15.3	0.4661	24	19

Ephemeris of Comet 1892 III, (Holmes) by Charles S. Benton.

(From A. J., No. 305).

	Gr. M. T. 1893	R. A. h	m	s	Decl. °	′	″	log Δ
Nov.	6.5	8	39	18.8	+ 35	10	52	0.5402
	8.5		39	45.3		13	42	
	10.5		40	06.6		16	47	
	12.5		40	22.6		20	08	
	14.5		40	33.2		23	43	0.5292
	16.5		40	38.4		27	33	
	18.5		40	38.1		31	36	
	20.5		40	32.3		35	52	
	22.5		40	21.0		40	20	0.5187
	24.5		40	04.0		45	00	
	26.5		39	41.5		49	48	
	28.5		39	13.3		54	47	
	30.5		38	39.3	35	59	52	0.5089
Dec.	2.5		37	59.7	36	05	04	
	4.5	8	37	14.4	+ 36	10	20	

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1893.		R. A. h	m	s	Decl. °	′	″	log Δ
Dec.	6.5	8	36	23.5	+ 36	15	42	
	8.5		35	27.0		21	03	0.5002
	10.5		34	25.2		26	24	
	12.5		33	18.0		31	43	
	14.5		32	05.1		36	59	
	16.5		30	48.3		42	08	0.4932
	18.5		29	26.0		47	09	
	20.5		27	59.1		51	59	
	22.5		26	27.8	36	56	38	
	24.5		24	52.4	37	01	03	0.4882
	26.5		23	13.0		05	13	
	28.5		21	30.0		09	04	
	30.5		19	43.6		12	38	
'94 Jan.	1.5	8	17	54.1	+ 37	15	46	0.4857

NEWS AND NOTES.

It is evidently hard for some of our subscribers to remember that this journal is published only ten times a year. July and September are uniformly vacation months. For twelve years this decimal system of publishing has been in use. The consecutive numbers of any volume will be found on the first page of the cover.

It is also latterly the custom of the publisher to inform subscribers promptly of the expiration of paid subscriptions, so that it may be known at once if it is their wish to continue the subscription. Neither of our publications will be sent to any one knowingly without orders for so doing. When letters of inquiry are sent, it is to be hoped that patrons will respond promptly that we may not be in doubt as to their wishes.

The second plate of the Corona by Professor J. M. Schaeberle which was to accompany his article in the last number was a failure on the part of the photogravure company entrusted to reproduce it. The bad feature in the case was that we were not notified of the failure until five days after the time the work was promised. Still worse, fifteen days later, we were informed that the company had given up the job entirely. We then did not have time to get the reproduction made elsewhere for this issue. We will try other artists in the hope of bringing out the beautiful long exposure plate by Professor Schaeberle which has been placed in our hands for this purpose. There is much of detail in the outer corona on this positive that we fear it will not be possible to reproduce. It is a worthy companion plate for that published last month.

Mounting of Telescope for Manila Observatory.—In the notes on a new Observatory for Manila published in our last issue, it was stated that the telescope being mounted for that Observatory by G. N. Saegmuller of Washington, D. C., is to be built on the same plan as the 12-inch at the new Naval Observatory and that it was designed by Professor W. Harkness of the Naval Observatory.

Mr. Saegmuller informs us that this statement is erroneous. We are glad to correct it. The facts are, the mounting for the Manila telescope differs very ma-

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terially from the 12-inch at the Naval Observatory, and secondly, Professor Harkness did not design it. As inspector of the work Professor Harkness made some changes in the plans and drawings which Mr. Saegmuller submitted: he placed the adjustment for azimuth and latitude on the head instead of on the base; he added the solar and lunar speed to the clock and placed the latter in the middle of the column instead of near the top.

In fact the 12-inch at the Naval Observatory is built on the same general plan as the two telescopes of the same size which Mr. Saegmuller designed and built for Georgetown College and Ladd Observatory respectively several years ago.

Professor Young observes Jupiter's Fifth Satellite.—I write to say that the new satellite of Jupiter was observed here by Professor Reed and myself on last Saturday night—Oct. 7-8. The atmospheric conditions were not very good, and the satellite was very faint—visible only at times, but then with perfect certainty. It was not possible to secure any micrometer measures, but we agreed in estimating the time of western elongation as between $12^h\ 35^m$ and $12^h\ 45^m$ eastern time. Marth's ephemeris would put it at $12^h\ 31^m$, so that the satellite appears to be some 9 or 10 minutes behind time. This agrees very closely with the result of Barnard's earlier observations of Sept. 15-16, which have just reached us in the *Astronomical Journal*.

C. A. YOUNG.

Princeton, N. J., Oct. 12, 1893.

Comet 1889 V in Jupiter's Satellite System.—In Nos. 302 and 303 of the *Astronomical Journal*, Dr. Charles Lane Poor discusses the orbit of Comet 1889 V with special reference to its passage through Jupiter's system in July, 1886. He concludes (No. 302, page 126) that the comet was 2.65 days within the system of Jupiter's satellites, and during this time it made nearly a complete circuit about the planet, passing over an arc of 313° of longitude. It passed its peri-Jove July 20.10 Greenwich mean time, at a distance of about 100,000 miles from Jupiter's center and in a longitude of about 275°.

It occurs to me that a comparatively slight force at this time would suffice if properly directed to cause a portion of the comet to be detached and to pursue an elliptical orbit around Jupiter. Such eruptive forces are common to comets at perihelion and would probably exist at such a close peri-Jovian passage. Does this furnish a possible origin of Jupiter's fifth satellite?

The small inclination of the comet's orbit, 6°, and the sufficiently close agreement of its peri-Jovian distance with the distance of the satellite, 112,000 miles, are favorable to this view, as also is the fact that the comet's motion around Jupiter was direct.

The question then arises—was the satellite in longitude 275° on this date? A question not so easily answered, since an uncertainty of 1 sec. in its periodic time, carried back to 1886 would cause an uncertainty of 36° in the satellite's longitude, to say nothing of perturbations by the other satellites which must be considerable.

J. A. PARKHURST.

Dudley Observatory.—When the new Dudley Observatory is completed we are promised a cut of it and detailed description. In the New York *Times* of Sept. 10 will be found a two-column article with a good etching of the Observatory building and a historical statement of the founding, growth and present rebuilding of this prominent Observatory.

While it is yet too "early to speak of the instruments of the Observatory in

detail, it may be said that the telescope is, however, mounted and nearly in adjustment, and has been tested on second-rate nights to an extent sufficient to show that its performance will be excellent. The objectives are 12.2 inches aperture, and there are three of these lenses. The back lens, crown glass, is common to both visual and photographic combinations, for each of which a flint front lens is provided. The focal length is 180 inches for the visual combination, and 162 inches for the photographic. There is a 4.5-inch guiding telescope attached to the main tube. The focal length of this 160 inches. There is also an excellent finder of 3 inches aperture."

"Among those who have contributed to the re-founding of the Observatory, as given in the paper before referred to, are Mrs. William L. Rice, Mrs. Catherine Gansevoort Lansing, Mrs. Anna Parker Pruyn and her daughter, Miss Huybertie Lansing Pruyn, of Albany, N. Y. Prominent among the other subscribers to the fund are Dean Sage, Samuel B. Ward, the heirs of Charles B. Lansing, James B. Jermain, E. G. Benedict, Grange Sard, Frederick Townsend, W. L. Learned, and John G. Myers, all well-known residents of Albany."

"The support of the Observatory is derived from an invested fund which yields an annual income of $5,000. The annual expenditure of the Government Observatory is about $60,000, and the annual income of the Harvard College Observatory is well above $40,000. The resources of the Dudley Observatory, notwithstanding its excellent facilities for work and its refined instruments, are thus manifestly far below those of an Observatory of the first rank in this respect. Yet it is the only astronomical Observatory in the imperial state of New York for which there is independent and permanent provision for astronomical investigations."

Dorothea Klumpke.—We regret exceedingly that our proof reader was so much at fault as to allow an error in the spelling of Dorothea Klumpke's name as it appears in her paper printed elsewhere. There is no good reason for a mistake in the name of one so well known as Dorothea Klumpke, the Directress of the Bureau of Measurements of Paris Observatory.

It may be added that the same person is undoubtedly meant in the following paragraph which we clip from one of our eastern papers:

"A recent issue of the Paris *Figaro* devotes half a column to an enthusiastic account of a young American girl named Klumpke, who has won for herself recognition as one of the most learned astronomers and most indefatigable and successful observers in France. Five years ago she was received as a pupil in the *Observatoire*. Since then a few other women have been allowed to join in the work carried on in that world-famous institution, but she was the first to whom the doors were opened, and for a long time she was the only one.

Parallax of Webb's Planetary Nebula, B. D. + 41°.4004.—During the summer of 1892 Dr. J. Wilsing of Potsdam Observatory began a series of photographs of Webb's planetary nebula, B. D. + 41°.4004, using the new refractor, with the intention of determining the parallax of the nebula. No. 3190 *Astronomische Nachrichten* gives an account of the measurements made. Six stars were used for comparison, and the distance of the nebula was measured from two of these stars, the others being used for finding the value of the measured distances in seconds of arc, etc.

The relative parallax comes out negative, and shows, as Dr. Wilsing thinks, that the distance of Webb's nebula from the Sun can not be assumed in any way to be less than the distances of both the eleven magnitude comparison stars.

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Dr. Janssen's Visit to the Observatory on Mont Blanc.—In *Nature* (October 5), is given a fuller account of Dr. Janssen's recent visit to the Observatory on Mont Blanc, than that found elsewhere under Astro-physical Notes. We copy that account for the sake of further facts of general interest found in it. It is as follows:

"We left Chamonix on September 8, at 7 A. M., and arrived at the summit on September 11, at 2:30 P. M. The Observatory was then in front of us. The construction has several floors, of which the framework, formed by large and massive beams crossed in all directions in order to ensure the rigidity of the whole, produces a deep impression upon the mind. One wonders how it has been possible to transport the edifice to this altitude and fix it on the snow. However, if the conditions offered by the hard, permanent, and little mobile snows of the summit are carefully considered, it is soon recognized that the snows are able to support very considerable weights,* and that they will be only slightly amenable to displacements, which will render it necessary to straighten again the construction which has been fixed upon them.

"On my arrival I made a rapid survey, and saw that the construction had not been sunk in the snow as much as I had stipulated of the contractors. I do not approve of this. My guides and myself then took possession of the largest underground room. I intended at first to fix the instruments for enabling observations to be commenced immediately, and the provisions were left on the Rocher-Rouge. This circumstance put us in a state of perplexity, for the weather suddenly became very bad, and we had to remain two days separated from the stores. The storm lasted from Tuesday until Thursday morning. Beautiful weather then set in and I was able to begin the observations.

"The observations have for their principal object the question of the presence of oxygen in the solar atmosphere. The Academy knows that I worked at this important point during my ascensions to the Grands-Mulets (3050 meters) in 1888, and at M. Vallot's Observatory in 1890.

"But the novelty of the observations of 1893 lies in the fact that they have been effected on the very summit of Mont Blanc, and that the instrument employed is infinitely superior to that of the two preceeding ascensions. At the first, in fact a Duboscq spectroscope incapable of separating the B group into distinct lines was employed, while the instrument about to be employed at the summit of Mont Blanc is a grating spectroscope (the dispersive piece of which I owe to the kindness of Rowland), with telescopes having a focal length of 0 75 and showing all the details of the B group. The circumstance is of considerable importance, for it may lead to the discovery, in the constitution of the group in question, of valuable elements for measuring in some way the effects of the diminution of the action of our atmosphere as one ascends into it, and, accordingly, to determine whether this diminution extends to total extinction at its limits. In fact we shall learn whether or no the double lines which make up the B group diminish in intensity as their refrangibilities diminish; that is, as their wavelengths increase.

"This circumstance may perhaps be employed with profit, if not to measure, at least to observe the diminution of the action of the selective absorbtion of our atmosphere. It has been ascertained that the most feeble doubles fade away one after the other as the atmosphere is ascended, that is to say, as the absorbing action is diminished, Thus, under ordinary circumstances, at the surface of our seas or upon our plains, thirteen or founteen doubles can be seen, not reckoning that which is known as the head of B.

* See *Comptes Reudus* for an account of experiments made at Meudon on the resistance of slightly compressed snow.

"But even at Chamonix, that is at an altitude of 1050 metres, the thirteenth double is very difficult to make out, and at the Grand Mulets (3050 m), it is only possible to see from the tenth to the twelth, while at the summit of Mont Blanc I could hardly go beyond the eighth.

"It is not to be supposed that we establish a proportionality between the numerical diminution of the doubles and that of the atmospheric action. The law is evidently of a much more complex character. But this diminution, especially when considered in connection with the experiments made with tubes full of oxygen, and able to reproduce the series of atmospheric phenomena to which we have referred, is sufficient for us to conclude that the B group would totally disappear at the limits of our atmosphere. It is remarkable, however, that if we take the coefficient 0.566 that represents the diminution of the atmospheric action at the summit of Mont Blanc according to barometric pressures and multiply it by thirteen—the number that represents the doubles clearly visible on the plain—we obtain 7·4 as the result; that is to say, very nearly the number (8) doubles that can be seen by me on the summit of Mont Blanc.

"This result is certainly remarkable, but I repeat that, in my opinion, it is only by the comparison with tubes reproducing the same optical conditions as nearly as possible, that any definite conclusions will be obtained. These comparative experiments have already been commenced in the laboratory of Meudon Observatory, and they lead to the same result, viz., the disappearance of the groups A, B, and α at the limits of the atmosphere. On account of the importance of the question, however, the experiments will be repeated and completed.

"The question arises as to whether the high temperatures to which solar gases and vapors are subjected are not capable of modifying the power of selective absorbtion, and particularly whether the absorbtion of oxygen which takes place in the sun's atmosphere would not be altogether different from that indicated by the experiments which have been made at ordinary temperatures.

"I have already instituted experiments with the idea of replying to this objection. I shall give an account of them to the Academy in due course, but I may say that the absorbtion spectrum of oxygen, either the line spectrum or the unresolvable bands, do not appear to be modified in an appreciable manner when the oxygen is raised to temperatures of about 400 or 500 degrees.

"On the whole, I think that observations made on the summit of Mont Blanc give a new and much sounder foundation to the study of the question of the purely telluric origin of the oxygen groups in the solar spectrum, and lead to the conclusions previously stated.

"Independently of these observations I have also given some attention to the transparency of the atmosphere of this almost unique station, and to the atmospheric phenomena which are included in such an extensive view, and across such a great thickness. I shall speak of this on a future occasion.

"The Observatory, of course, is not completed. There yet remains much to be done independently of interior arrangements and the installation of the instruments; but the great difficulty has been overcome, for we are free to work, and no longer have to reckon with the snowstorms; the rest will follow in due course.

"I hope that the Observatory will soon be able to offer a much more comfortable sojourn than I have had there; but that will depend upon the weather. Be this as it may, I regret nothing. I strongly wished to see our work in position, and still more fervently desired to inaugurate it by observations which are ever in my mind. I am fortunate at having been able to realize my desires in spite of some difficulties."

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The Change of Sensitiveness in Dry Plates.—We quote some exceedingly interesting and useful remarks on the subject of dry plates in astronomical photography from a paper* by Professor Max Wolf of Heidelberg University:

"* * It is not a pleasant experience to give an eight or nine hours' exposure to what is believed to be a highly sensitive plate, and then to find on development that the whole work has been thrown away, because the plate was really quite an insensitive one. The photographer who has had this experience repeated several times (as I have), very soon learns to become cautious. The only reliable test of sensitiveness, however, as I may here remark, is comparison by actual exposure to stars, the ordinary sensitometer tests being much too uncertain.

"Special caution is necessary in dealing with fresh plates. In the early part of my work I always noticed that new plates received from the makers were uniformly less sensitive than the previous ones, and that it was necessary to expose them a much longer time, so that it almost seemed as if the manufacture of dry plates was retrograding. * * The peculiarity is so strongly marked that during the last winter I was hardly able to obtain the same objects on a new lot of Lumiére plates that I had previously obtained with the last plates of the same make, even with a three-fold greater exposure. * *

"I had, indeed, known earlier than this that plates changed somewhat in sensitiveness when stored, but I could hardly expect that the change would amount to so much as a three-fold increase; and yet it was so. After five months the new Lumiére plates, at first so slow, were as sensitive as the preceding ones, and exceeded in sensitiveness all my other plates. A similar change took place in those of other makers. * *

"The sensitiveness does not by any means increase indefinitely with the time. On the contrary, it soon reaches a maximum, which persists for some time, and after this the sensitiveness diminishes. * *

"For Lumiére plates this time has been found to be from five to seven months after manufacture. By taking advantage of this fact much can be gained; sometimes, as I have said, an increase in sensitiveness of three or four times. * *

"From the foregoing the astronomer may take warning never to assume that plates made from the same emulsion are equally sensitive if they are used at different times. * * For the same reason it is also very difficult to determine beforehand what exposure should be given in order to obtain stars of a certain magnitude. It is quite impossible to do this (leaving out of the question changes in the transparency of the air), without taking into account the age of the plates."

W. W. C., in Pub. 31, A. S. P.

To the Editors of Astronomy and Astro-Physics:

GENTLEMEN: In your number for August, 1893, page 664, is an extract from a letter of Dr. Joseph Morrison, in which occurs the following sentence:—"In 1884 I was elected a Fellow of the Royal Astronomical Society of England, a higher position than Professor Newcomb has in that learned Society." . . As it is possible that the words may give rise to a misapprehension, among those unacquainted with the constitution of our society, as to the relative position of our Fellows and Associates, I beg to point out that associates, of whom Professor Simon Newcomb is one, must be, in the words of the Bye-Laws, "*persons eminent in the science of Astronomy.*" They are elected *honoris causa* on the invitation of the Council, in recognition of their distinguished services to astronomy.

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* The English translation in ASTRONOMY AND ASTRO-PHYSICS for August, from the original in Eder's *Jarbuch für Photographie und Reproductions-Technik.*

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Our Fellows, on the other hand, are, persons "desirous of admission into the Society," from whom no other scientific qualification is required than a genuine interest in astronomy. They have to state that they are desirous of being elected Fellows, and on election they pay an admission fee and annual contribution.

I should further state that Dr. Morrison is no longer a Fellow of the Society. His name was removed from the list of Fellows in 1891 in accordance with the Bye-Laws of the Society.

I am gentlemen,
Your obedient servant,
H. H. TURNER, Secretary.

Professor George E. Hale of Chicago University, editor of the department of Astro-Physics, is already on his way to Europe. His purpose is to spend some time in the study of particular themes by original research pertaining to his dement of science.

Double Star Astronomy.—In the current numbers of *The Observatory* Mr. T. Lewis is giving a series of articles on Double Star Astronomy. In the October number he writes on methods of calculating orbits of binary stars and on personal equation in double star measures. These articles will perhaps do good in exciting new interest in double star measurements, a field of work which has largely been adandoned for the newer work in photography and spectroscopy. It is to be hoped that a few observers with suitable telescopes will keep to this work, for we are now just getting to the point where the orbits of binaries can be computed with a reasonable degree of accuracy.

New Asteroids.—Since our last note on the asteroids in May number of ASTRONOMY AND ASTRO-PHYSICS, fifteen have been discovered, all by means of photography. 1893 AF has been identified with 158 Corona.

Designation.	Discovered by	Date of Photograph.	Magnitude.
1893 Y	Wolf	April 14	13
Z	Charlois	May 19	12
AA	Charlois	" 20	11
AB	Charlois	" 20	13
AC	Charlois	July 14	12
AD	Charlois	" 16	11
AE	Borrelly	" 5	12
AF	Charlois	Aug. 11	12
AG	Charlois	" 17	11
AH	Charlois	" 19	10
AJ	Charlois	Sept. 15	12
AK	Charlois	" 18	12
AL	Charlois	" 18	11
AM	Charlois	" 18	12
AN	Charlois	" 20	11.5

Astronomical and Physical Society of Toronto.—The last regular meeting of the Astronomical and Physical Society of Toronto was held in the society's rooms, No. 19 McGill street. The attendance was large and included several strangers, who were made welcome. The chair was occupied by Dr. Larratt W. Smith, Vice President. A new feature is the presentation to the society by outsiders of books and valuable papers, some of them rare. At a recent meeting a copy of Ferguson's Astronomy, printed in 1758, in two volumes, became the property of the society, but the third volume, containing the plates, was wanting. By a

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curious coincidence the volume necessary to make the set complete was donated at the last meeting by a friend who was unaware of the earlier presentation. The first number of Popular Astronomy, an illustrated monthly magazine published entirely in the interests of amateur astronomers, was laid on the table. It was highly praised by those members who examined it. It seems to be all and more than the publishers promised. The contributions are by the best writers and include easy papers on "Constellation Study," "The Spectroscope and some of its Applications," "The Moon," "The Asteroids and Their Relation to the Planetary System," "Concerted Observation of the Aurora," "Jupiter's Comet Family," "Astronomy with a Small Camera," "Nebula and Comet Seeking," "A Lesson on the Harvest Moon," "Shooting Stars and How to Observe Them." In addition, there are valuable planet notes, planet tables, comet notes, general notes, and a description of the "Face of the Sky" for the month of issue. This monthly deserves a large circulation. It is published by Professor W. W. Payne, director of the Goodsell Observatory, Northfield, Minnesota. Under the head of observations, Mr. A. Elvins announced that Finlay's comet, a telescopic object, is visible near "the beehive" in constellation Cancer. Mr. J. R. Collins showed several excellent negatives of the Sun and Moon taken with a two-inch telescope, and Mr. A. F. Miller handed in a drawing of the solar disc, September 24, at 11 o,clock, showing accurately the position of the sun-spots and faculæ by projection, and, at the same time, the position angles and shapes of the numerous red prominences scattered around the Sun's limb and visible by the spectroscopic method. Mr. John A. Copland objected to the explanation that the obscuration of the Sun on Friday, the fifteenth of September, was due to smoke at high altitudes and said that observation with a telescope and good field glass "revealed dust-specks gyrating in the atmosphere, as do those observed in a Sun ray," and that "these motes were of uniform color, dull grey." He suggested the cause might have been particles floating in the air, into which they had been ejected by Mount Colima volcano, in Mexico, which some days previously had broken out with a terrific explosion.

BOOK NOTICES.

The Science of Mechanics; a Critical and Historical Exposition of its Principles, by Dr. Ernst Mach, Professor of Physics in the University. Translated from the Second German Edition by Thomas J. McCormack. Two hundred and fifty cuts and illustrations. Chicago: The Open Court Publishing Co., 1893, pp. 534. Price cloth $2.50.

Mach's Mechanics was first published in 1883 The author's aim then was to produce a book on this subject that should be treated as one of the physical sciences, and not primarily as a branch of mathematics. In 1888 the book passed through its second edition, and the translation before us is from this later edition.

In the introduction the general idea of the science of mechanics is stated. Instruction, knowledge and its relation to investigation and mechanical experiences are spoken of as very old, and references and illustrations from the Egyptian and Assyrian monuments are given. A brief discussion, concerning the origin of science, the nature of knowledge and its mode of communication, occupies the remaining part of the introduction.

The text proper begins with the development of the principles of statistics using the lever as the first theme. The new feature of treatment is the prominence given to the historical side of it. The earliest mechanical researches relating to statistics referred to were those of Archimedes, and enough is given to indicate his methods of work and the character and value of his results. His

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mode of view was modified by Galileo, but it was the modern Lagrange who showed how to study the lever in a concise way that revealed a practical mathematical perception. Huyghens' method is also presented and illustrated and with the others discussed by the author. This subject occupies fifteen pages. In a similar way are presented the inclined plane, the composition of forces, and virtual velocities, with a review of statistics as a whole.

The author then considers the principles of statistics in their application to fluids, also to gaseous bodies.

The second chapter treats of the development of Dynamics, first noticing Galileo's achievements, then the dispute of the Cartesians and Leibnitz followed by a description of Morin's apparatus. Under a second title are found the achievements of Huyghens presented in an attractive way, especially the pendulum clock that goes by his name.

A third title gives the work of Newton in discovering universal gravitation which is detailed quite fully. Also, under other sections are given Newton's views of time, space and motion, a synoptical critique of the Newtonian enunciations, and a retrospect of the development of dynamics.

The three remaining chapters are devoted respectively to the extended applications of the principles of mechanics and the deductive development of the science; the formal development of mechanics, and the relations of mechanics to other departments of knowledge. The book closes with an appendix, a chronological table of a few eminent inquirers and of their more important mechanical works, and an index.

We deem the book a very useful one, in that it gives the student some idea of the German method of study and topical work in elementary physics which will be helpful to those who are not already acquainted with them.

An Academic Arithmetic for Academies, High and Commercial Schools. By Webster Wells, S. B., associate Professor of Mathematics in the Massachusetts Institute of Technology. Messrs. Leach, Shewell & Sanborn, publishers, Boston, New York and Chicago, pp. 339.

This new book is the lowest in the Wells' mathematical series which has already become widely and favorably known. The more elementary part of the subject is about the same as usually found in good books in arithmetic. The statement of principle or rule is concise and the matter is arranged on the page in systematic way. Factoring and the tests by nines, afford useful practice. Young students ought to form habits of testing results in daily exercise. The illustrations in fractions are better than usual and will wear in the class-room. The attention given to the metric system and its applications and to mensuration in general is noteworthy. The practical bearing given to percentage, interest, discount, exchange, stocks, bonds and kindred subjects are some of the other points in its favor. The author has evidently taken pains in the selection of the matter and in the arrangement of this text-book to adapt it well to the place for which it is designed.

The Principles of Elementary Algebra. By N. P. Dupuis, M. A., F. R. S. C., Professor of Pure Mathematics in the University of Queen's College, Kingston, Canada. New York, Messrs. Macmillan and Co., 1892.

This book is intended to fill the place of an intermediate algebra, in the sense that it is not prepared for beginners, nor those who are proficient in the branch. Although this is true, it covers a fairly wide range of subjects from the elemental operations to the subject of determinants. The chapter on factors and factorization is suggestive of good methods for class drill. The titles of some of the chapters of this book give a fair idea of the breadth of the themes as they are presented. Fractions, ∞ and 0; ratio, proportion, variation and generalized proportion; concrete quantity, geometrical interpretations, the graph; undetermined coefficients and their applications; logarithms and exponentials; series and interpolation and elementary determinants.

The geometrical interpretation and the graph, so called, are certainly very helpful methods to aid the student in getting the meaning of some algebraic operations that are usually rather obscure.

The clear statement of points, the numerous examples and the range of subjects brought into a compass of 336 pages speak favorably for this text-book.

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PUBLISHER'S NOTICES.

The subscription price to ASTRONOMY AND ASTRO-PHYSICS in the United States and Canada is $4.00 per year in advance. For foreign countries it is £1 or 20.50 marks per year, in advance. Recent increase in price to foreign subscribers is due to increase of postage because of enlarged size during the year 1892. Messrs. Wesley & Son, 28 Essex Street, Strand, London, are authorized to receive subscriptions. Payment should be made in postal notes or orders or bank drafts. *Personal checks for subscribers in the United States can not longer be received.*

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James E. Keeler, Observatory, Allegheny, Pa.; Henry Crew, Northwestern University, Evanston, Ill.; Jos. S. Ames, Johns HopkinsUniversity, Baltimore, Md.

All matter or correspondence relating to General Astronomy, remittances, subscriptions and advertising should be sent to Wm. W. Payne, Publisher and Proprietor of ASTRONOMY AND ASTRO-PHYSICS, Goodsell Observatory of Carleton College, Northfield, Minn.; and the Associate Editors for General Astronomy are: S. W. Burnham, Government Building, Chicago Ill.; E. E. Barnard, Lick Observatory, Mt. Hamilton, Cal., and H. C. Wilson, Goodsell Observatory, Northfield, Minn.

Manuscript for publication should be written on one side of the paper only and *special care should be taken to write proper names and all foreign names plainly*. All drawings for publication should be *smoothly* and *carefully* made, in *India Ink* with lettering well done, because such figures are copied exactly by the process of engraving now used. If drawings are made about double the size intended for the printed page, better effect will be secured in engraving than if the copy is less in size. It is requested that manuscript in French or German be typewritten. If requested by the authors when articles are sent for publication, *twenty-five* reprint copies, in covers, will be furnished free of charge. A greater number of reprints of articles can be had if desired, at reasonable rates.

Rates for advertising and rates to news agents can be had on application to the publisher of this magazine.

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